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XII. *A Supposed Relationship between Sunspot Frequency and the Potential Gradient of Atmospheric Electricity.* By C. CHREE, *Sc.D., LL.D., F.R.S.*

RECEIVED NOVEMBER 9, 1922.

ABSTRACT.

In a recent Paper in *Terrestrial Magnetism*, Dr. L. A. Bauer draws the conclusion that both the range of the diurnal inequality of atmospheric electricity potential gradient and the mean value of the element for the year increase and diminish with sunspot frequency. This conclusion was based on observational data from the Ebro Observatory, Tortosa, Spain, between 1910 and 1920.

The present Paper investigates the subject more mathematically, employing Kew electrical data from two periods of years, in addition to the Ebro data utilised by Dr. Bauer. Magnetic data from Kew Observatory for the same periods are similarly treated, so as to have parallel results from an element for which the sunspot relationship is generally admitted.

The results obtained are on the whole not incompatible with Dr. Bauer's conclusion, but they indicate that if a relationship of the kind supposed exists, the sunspot influence must be very much less in the case of atmospheric electricity than in that of terrestrial magnetism.

IN a recent number of *Terrestrial Magnetism** Dr. L. A. Bauer announces the following conclusion :—

"*f.* The potential gradients of earth currents and of atmospheric electricity apparently vary during the sunspot cycle . . . the latter increasing with increased sunspot activity. The diurnal ranges of the potential gradients of earth currents, as well as of atmospheric electricity, just as is the case for the diurnal variation of terrestrial magnetism, increase with increased sunspot activity " (*l.c.*, p. 30).

The phenomena in terrestrial magnetism to which Dr. Bauer refers are generally accepted as holding all over the earth. We should thus naturally suppose his conclusions respecting atmospheric electric potential to be equally general. But the potential gradient results he dealt with were from only one observatory (Ebro, Tortosa, Spain), and he may have intended his conclusions to be limited to that one station. It is desirable in any case to see what happens at other stations, because it would be a very important step indeed if a general relationship of the kind indicated could be established, at all as decided as that between sunspot frequency and terrestrial magnetism. This has led me to see what further light can be thrown on the question by the Kew Observatory records of atmospheric electricity potential gradient.

A Kelvin water-dropping electrograph has been in action at Kew Observatory with little interruption since 1862. To translate the measurements of the photographic records into absolute measure, it is necessary to take absolute observations in an open spot, and to compare the voltages so measured with the corresponding curve readings. A system of absolute observations was not introduced at Kew until 1898. Since then it has been in regular operation. But there was an improvement in 1910, both in the apparatus and in the site of the absolute observations.

* Vol. 27, pp. 1-30.

These improvements showed that the previous measurements gave too low a conversion factor, consequently the previously published voltages were too low. A conclusion was reached as to the factor by which the older figures should be multiplied to make them comparable with those based on the new apparatus. There had been no change in either apparatus or methods from 1898 to the end of 1909. So far as relative values are concerned, the data from these years are as satisfactory as the more recent data.

It appeared desirable to consider alongside the electrical data magnetic data for the same periods. The magnetic results will show what happens in a case where a relation with sunspot frequency is generally admitted. It also seemed desirable to make certain that the solar influence was not abnormally high or low during the periods under review.

The employment of magnetic data partly determined the periods selected. In 1910 Kew Observatory came under the Meteorological Office, and some changes were then introduced. Up to the end of 1910 the non-cyclic changes were not eliminated from the published diurnal inequalities of the magnetic elements, and the figures went only to 1γ in H (horizontal force) and to $0{\cdot}1'$ in D (declination). From 1911 onwards non-cyclic changes were eliminated, and the inequality figures went to $0{\cdot}1\gamma$ in H and $0'{\cdot}01$ in D. This led to the adoption of 1911 to 1921 for one period. This is almost identical with the period 1910 to 1920, on which Dr. Bauer's conclusions rest. The difference between the mean sunspot frequencies for the two periods is only 0·5.

Dr. Bauer did not employ the actual range of the regular diurnal inequality, but a quantity c_r given by

$$c_r^2 = c_1^2 + c_2^2 + c_3^2 + c_4^2 + c_5^2 + c_6^2,$$

where c_1, c_2, &c., are the amplitudes of the Fourier waves of periods 24, 12, &c., hours.

The calculation of Fourier coefficients for a number of years is troublesome. There were, however, available values of c_1 and c_2 derived from the mean diurnal inequality of potential gradient at Kew Observatory for each year from 1898 to 1912.

This suggested the period 1898 to 1912. An objection might, however, have been raised to the last three years of this period owing to the change of apparatus mentioned above; accordingly the period was confined to the twelve years 1898 to 1909.

To obtain values of c_r exactly equivalent to Dr. Bauer's would have entailed the calculation of the shorter period Fourier waves. The prospective labour was only one of the reasons for not doing this. If c_6 represents, as follows from a remark of Dr. Bauer's, a 4-hour wave, c_5 must answer to a period of 24/5 hours. As this is not an exact multiple of an hour, and the Kew curves had been measured at 1-hour intervals, the calculation would have presented difficulties. When Fourier waves are calculated from individual years' data at Kew Observatory, there are irregular variations in the amplitudes and phase angles from year to year even in the 8-hour and 6-hour waves, which suggests that accident plays rather a considerable part. This irregularity can be recognised in Dr. Bauer's Ebro results, especially those connected with c_3, c_5 and c_6. The contribution to the annual values of a c_r of Dr. Bauer's type from the shorter period waves at Kew would undoubtedly have pos-

sessed a considerable accidental element. Lastly, the contribution from these smaller terms, relatively considered, would have been trifling. Values of c_1, c_2, c_3, c_4 calculated at Kew for a group of years were as follows, in volts/metre:—

$$c_1=23{\cdot}2,\ c_2=48{\cdot}3,\ c_3=8{\cdot}0,\ c_4=6{\cdot}1.$$

Thence we get $(c_3^2+c_4^2)/(c_1^2+c_2^2)=1/28$ approximately.

The value of $(c_5^2+c_6^2)/(c_1^2+c_2^2)$ would naturally be considerably less, and at Kew $c_3^2+c_4^2+c_5^2+c_6^2$ would probably represent only about 5 per cent. of c_r^2 as defined by Dr. Bauer. Mathematical considerations alone suffice to show that if we take two c_r's, one as defined by Dr. Bauer, the other defined as the equivalent of $c_1^2+c_2^2$, neither could show a pronounced solar influence without the other also doing so. This is supported by physical considerations. While the phase angles given by Dr. Bauer for the less important waves, especially φ_3, φ_5 and φ_6 show considerable fluctuations, these are irregular and not suggestive of any decided solar influence. On the other hand, the fluctuations of the phase angles of the principal waves are small. The type of the diurnal inequality would seem to be nearly constant. If the type were quite constant, c_r, whether as defined by Dr. Bauer or as the equivalent of $(c_1^2+c_2^2)^{\frac{1}{2}}$, would bear a constant ratio to the range of the inequality, and the precise value of the constant would be immaterial for the present purpose. Accidental features would no doubt present themselves differently in the annual values of the two forms of c_r and in those of the inequality range, but it seems reasonably certain that any decisive solar influence would manifest itself to a similar extent in the three quantities. It was accordingly decided to take as one of the electrical quantities for the period 1898 to 1909, a c_r defined by

$$c_r^2=c_1^2+c_2^2.$$

The data employed in the investigation are given in Tables I. and II. All the data are given as differences + or − from the mean value of the quantity for the period considered. By comparing the signs and sizes of the corresponding sunspot and magnetic or electrical data in the same column, even the unmathematical reader can form a judgment as to the existence or non-existence of a close connection. The letter S is attached to sunspot data (Wolfer's frequencies), and the letters M and E to magnetic and electrical data respectively. The suffixes 1 and 2 serve to distinguish the data in Table I. from those in Table II. The last column gives in each case the mean value of the quantity for the period considered. S_1, S_2 and S'_2 refer respectively to the periods 1898-1909 (mean sunspot frequency 32·8), 1910-20 (mean frequency 39·0) and 1911-21 (mean frequency 39·5). M_1 and M_2 relate to the range R_h of the mean diurnal inequality for the year of horizontal force at Kew, while M'_1 and M'_2 relate to the range R_d of declination The noncyclic changes were duly allowed for in all cases where this had not already been done. The magnetic data were derived from the international quiet days, five a month. It is known that the sunspot influence on Terrestrial Magnetism is shown by all days, quiet or disturbed, though not necessarily to the same extent.

E_1 refers to the $c_r\equiv(c_1^2+c_2^2)^{\frac{1}{2}}$ at Kew calculated from the mean diurnal inequality of the year, the mean value of c_r from 1898 to 1909 being 54·3 volts per metre. E'_1 refers to the mean value of the potential gradient P for the year at Kew, on a scale supposed identical with that used in later years, but dependent on a different apparatus.

TABLE I.—*Sunspot, Magnetic and Electrical Data.*

	1898	1899	1900	1901	1902	1903	1904	1905	1906	1907	1908	1909	Mean Value
S_1	−6·1	−20·7	−23·3	−30·1	−27·8	−8·4	+9·2	+30·7	+21·0	+29·2	+15·7	+11·1	32·8
M_1	−3	−4	−4	−5	−5	−3	−1	+7	+4	+6	+4	−1	25γ
M'_1	−0·1	−0·3	−0·5	−0·8	−1·0	+0·1	−0·1	+1·1	−0·1	+0·5	+0·9	0·0	7′·0
E_1	+4·1	+8·7	−2·1	−10·4	−9·2	+0·3	−5·6	+8·6	+5·3	−1·3	−2·3	+4·0	54·3*v/m*
E'_1	+4	+38	−35	−6	−27	+5	+15	+15	−6	+7	−21	+9	304*v/m*

TABLE II.—*Sunspot, Magnetic and Electrical Data.*

	1910	1911	1912	1913	1914	1915	1916	1917	1918	1919	1920	1921	Mean Value
S_2	−20·4	−33·3	−35·4	−37·6	−29·4	+8·4	+16·4	+64·9	+41·6	+24·6	−0·3	...	39·0
E_2	−2·8	+0·1	−3·7	−1·0	−1·0	+1·6	−1·1	+4·9	+1·8	−1·2	+2·2	...	25·1*v/m*
S'_2	...	−33·8	−35·9	−38·1	−29·9	+7·9	+15·9	+64·4	+41·1	+24·1	−0·8	−14·8	39·5
M_2	...	−6·0	−5·0	−7·1	−3·0	−0·4	+4·8	+8·8	+4·9	+2·8	+3·1	−2·6	25·2γ
M'_2	...	−1·81	−1·29	−0·78	−1·52	−0·35	+1·08	+2·53	+1·58	+0·87	+0·26	−0·58	7′·65
E'_2	...	−29	−30	+5	+15	+24	+37	+24	+16	+1	−15	−49	330*v/m*
E''_2	...	+6	+1	+12	+21	+25	+3	+6	−9	−24	−26	−16	148*v/m*

E_2 refers to the value of c_r for the Ebro Observatory, as given by Dr. Bauer. The mean value accepted, however, 25·1 volts per metre, differs slightly from that assigned by Dr. Bauer, whose value answered apparently to the arithmetic mean values for c_1, c_2, &c., and was not the arithmetic mean of the values of c_r itself. Dr. Bauer had not made any exact numerical evaluation of the sunspot influence, and it seemed desirable to have numerical results for comparison with those for Kew.

E'_2 represents the mean annual value P of potential gradient at Kew, corresponding exactly to E'_1, except that it refers to a later epoch. E''_2 represents the range R_p in the mean diurnal inequality of potential gradient at Kew, and is thus a quantity of the same species as E_1 and E_2.

The data were all treated strictly mathematically. The values of b in the "regression equation" $y=bx$—where x and y are corresponding departures of the independent and dependent variables X and Y from their mean values—were calculated in the usual way from the equation

$$b=\Sigma xy \div \Sigma x^2,$$

and the "correlation coefficient" r was similarly calculated from

$$r=\Sigma xy \div (\Sigma x^2 \, . \, \Sigma y^2)^{\frac{1}{2}}.$$

I have preferred to exhibit the results in formulæ of Wolf's type

$$\text{Quantity}=a+bS,$$

where S is the sunspot frequency of the year.

Take, for example, the case of M_2 (*i.e.*, the horizontal force inequality ranges in Table II.). The original form of the regression equation

$$R_h=25{\cdot}2+0{\cdot}144\ (S-39{\cdot}5)$$

reduces at once to

$$R_h=19{\cdot}5+0{\cdot}144S.$$

In this case $a\equiv19{\cdot}5\gamma$ is the range calculated for zero spot frequency. $100b\equiv14{\cdot}4\gamma$ represents the increase in the range associated with a sunspot frequency of 100, and

100 b/a is the ratio borne by this increase to the range found for the ideal year of no spots. The frequency usually falls short of 100 for the sunspot maximum year, and hardly ever falls to zero for the sunspot minimum year. Thus, if 100 b/a is small compared with unity, the sunspot influence is trifling.

Sunspot influence may, of course, be small and yet real. The criterion as to the reality of the correlation is r. If the correlation is close, r should approach unity. A small value of r implies that there is little if any real correlation between the two quantities compared.

The results of the calculations are a follows, magnetic data for Kew for the earlier period 1890-1900 being included for purposes of comparison :—

Quantity.	Period.	Formula.	100 b/a	r
M_1	1898—1909	$R_h=19+0{\cdot}188\ S$	0·99	0·94
M'_1	1898—1909	$R_d=6{\cdot}2+0{\cdot}0237\ S$	0·38	0·84
M_2	1911—1921	$R_h=19{\cdot}5+0{\cdot}144\ S$	0·74	0·95
M'_2	1911—1921	$R_d=6{\cdot}12+0{\cdot}0387\ S$	0·63	0·96
	1890—1900	$R_h=18{\cdot}1+0{\cdot}194\ S$	1·07	...
	1890—1900	$R_d=6{\cdot}10+0{\cdot}0433\ S$	0·71	...
E_1	1898—1909	$c_r=50{\cdot}1+0{\cdot}129\ S$	0·26	0·46
E'_1	1898—1909	$P=297+0{\cdot}225\ S$	0·08	0·24
E_2	1910—1920	$c_r=23{\cdot}1+0{\cdot}051\ S$	0·22	0·72
E'_2	1911—1921	$P=313+0{\cdot}440\ S$	0·14	0·55
E''_2	1911—1921	$R_h=148-0{\cdot}118\ S$	−0·08	0·24

Taking the magnetic results first, we see that the different periods supply very nearly the same values for a, the range answering to zero sunspots, but differ rather notably in their values for b. When a formula of the type $R=a+bS$ is applied to the individual 12 months of the year, for a period of years, b/a is found to possess a large annual variation. In both declination and horizontal force it is higher for winter than summer months, but to an extent varying with the element. Two years having the same mean sunspot frequency may have the highest frequencies at different seasons. Thus a sensible variation in the values o' b/a derived as above from different periods is not surprising. But the value of b/a deduced in the case of M'_1 is certainly on the low side, and the same is true of the corresponding value of r. This seems due in part to the inequalities having been taken out only to $0'{\cdot}1$. It so happened that the mean value of R_d for the period was almost equally distant from $6'{\cdot}9$ and $7'{\cdot}0$. The negative values appearing for M'_1 in Table I. for the years 1904 and 1906 are attributable to $7'{\cdot}0$ being taken instead of $6'{\cdot}9$. The sunspot maximum in the period 1898 to 1909 was also poorly developed, and was of a somewhat exceptional character, there being two nearly equal frequencies in 1905 and 1907, with a considerably lower value intercalated. The magnetic diurnal inequalities for some reason were poorly developed in 1906 and also in 1904.

While the values of b/a for the M quantities in Tables I. and II. may be on the whole a little below average, they are not outstandingly so, and the mean value of r for the four exceeds 0·92. There is thus no reason to anticipate a specially poor manifestation of the sunspot influence in the case of the electrical quantities, if a substantial real influence exists.

The resulting formulæ for E_1 and E_2 support one another, the values obtained for b/a being closely alike. Also the values of r, though markedly lower than those found for the M elements, are at least not very small. But E''_2 is, as explained

above, a quantity of exactly the same type as E_1 and E_2, and applies to a period of years in which sunspot maximum was specially well developed, and in its case not only are b/a and r low, but the former is even negative.

In the case of E'_1 and E'_2, both representing the mean annual value of potential gradient, the results are again consistent with a positive sunspot influence; but the values of b/a are both very small, and the values of r are on the low side, especially that for E'_1.

The conclusion drawn as to the E elements will naturally depend on the temperament of the judge. The balance of the results derived from the Kew data is admittedly on the side of a solar influence such as Dr. Bauer describes. But the results are insufficient to prove that the influence exists, while they certainly do prove that it must, if existent, be of a very different order from the influence seen in the case of the magnetic elements. If, for example, we take the Kew case E_1, which supplied the largest value of b/a, and compare its yearly entries in Table I. with the corresponding entries for S_1, the lack of parallelism is at once apparent. The E_1 value in 1904 is not merely negative but numerically substantial, and the absolutely largest positive value occurs in 1899, when sunspot frequency was much below its average.

The E case most favourable to Dr. Bauer's conclusion is that of the Ebro, E_2; for, while the value of b/a is a trifle smaller than in the corresponding case, E_1, at Kew, the value of r is considerably bigger. But it is the divergence of r from unity that has to be considered, and that divergence in the case of E_2 is fully five times as large as it is in the case of the Kew magnetic elements for what is practically the same period. There are other not very re-assuring features in the case of E_2. The values of b/a and r are much influenced by the outstandingly high value of c_r in the year of sunspot maximum. The value of xy contributed by that one year represents fully half the net value of $\Sigma\,(xy)$ for the eleven years. The contribution from the sunspot maximum year, if a real correlation exists, ought of course to be considerably the largest, but its pre-eminence is relatively much greater in the case of E_2 than in the case of any Kew magnetic element. One would like to feel quite sure that there is no reasonable explanation of a more local nature than sunspots for the phenomenon. This is all the more so because the range of the diurnal inequality of potential gradient in 1917 was not outstanding at Kew, while at Eskdalemuir it was less for the quieter days than in any other year from 1915 to 1918.

The irregularities visible when we compare the sequence of values of E_2 and S_2 in Table II. are similar to those observed in the Kew E data. There are substantial deficiencies in E_2 in 1916 and 1919, years of considerable excess in S_2. On the other hand, the second largest excess in E_2 occurs in 1920 when S_2 was below the mean, and there is a slight excess of E_2 in 1911 when the deficiency in S_2 is very large.

A few general remarks seem called for.

There seems to be at least a general parallelism between simultaneous magnetic phenomena all over the globe. A year that is characterised by great disturbance or large diurnal variations at one station is, so far as is known, similarly characterised everywhere. There is thus nothing surprising in a *general* relation between sunspot frequency and the amplitude of the diurnal inequality of a magnetic element.

But, so far as I am aware, no such general similarity has been recognised between simultaneous electrical phenomena at different stations. Phenomena at Kew and Eskdalemuir, for instance, show no obvious parallelism. The electrical phenomena

at a particular station are unquestionably much dependent on the weather and other purely local conditions. A year may be very dry at one station and very wet at another. Thus to establish satisfactorily any *general* relation between sunspot frequency and atmospheric electricity it would be necessary to have simultaneous data from a number of stations in different parts of the world.

Again, it has been found, at least at Kew Observatory, that the absolute value of potential gradient is considerably dependent on the visibility, and so purity, of the atmosphere. Various authorities have described widespread contamination of the atmosphere by dust arising from outstanding volcanic eruptions. There may, for all we know, be a correspondingly widespread effect on the potential gradient. A real solar influence corresponding to a value as small as 0·2 in 100 b/a might well be swamped by such an influence, or a fictitious effect of this size might be introduced. It thus seems very desirable that electrical data from several sunspot cycles should be contrasted.

There are special difficulties in the way of securing uniformity in electric potential data. The voltages actually recorded are dependent on instrumental conditions which it is hardly possible to keep absolutely constant. To secure really good results, it is necessary to have accurate and not infrequent scale value determinations, and also to determine with precision at not too long intervals the factor for reduction to the infinite plane. Instrumental troubles, especially inferior insulation, call for incessant care. The difficulties of securing really comparable data for a period of years are, in short, much more serious than in the case of terrestrial magnetism when a good magnetograph is available.

There is also a difficulty in the rapid large oscillations not infrequently encountered in potential gradient. If the sensitiveness of the electrograph is that appropriate for quiet days, the trace on disturbed days is apt to go off the sheet, or to be too faint to decipher. It is thus practically impossible at the ordinary station dependent on only a single electrograph to employ all days when calculating mean values or diurnal inequalities. Mean annual values and diurnal inequalities vary according to the choice of days. At Kew a choice is made of a uniform number of days, 10 a month, free from negative potential—the usual accompaniment of disturbed weather—in hopes of securing a fair representation of fine weather conditions. But how far the results can be regarded as fairly representative of the *year* must be a matter of opinion. Highly disturbed days seem also to be omitted at the Ebro Observatory, as the results are said to be derived from days of characters 0 and 1—highly disturbed days are usually described as of character 2—but the number of such days will naturally vary with the season. What the result would be if the Kew and Ebro procedures were interchanged no one can say.

In view of the interest which attaches to the theoretical question as to what is the origin of the negative charge on the earth's surface, evidenced by the prevailing positive sign of the potential gradient, it may be pointed out that if the mean annual value of potential gradient waxes and wanes all over the earth with sunspot frequency, so also must the earth's total negative charge.

With respect to Dr. Bauer's reference to earth currents, the well-known relation of magnetic storms and earth currents renders an 11-year period in the latter almost a foregone conclusion. This was pointed out in the article on Earth Currents in the *Encyclopædia Britannica*, 11th Edition (Vol. VIII., p. 815, section 8).

DISCUSSION.

Dr. A. Russell, in expressing the thanks of the meeting for an interesting and instructive Paper, said that few realise the very high value of the potential gradient which normally characterises their physical environment, and which may have important biological effects, not only in connection with electro-culture but also in human psychology. The "correlation coefficient" which Dr. Chree introduced was not familiar to all physicists, and appeared to be of very great interest as a test of the inter-relation of different phenomena.

Dr. D. Owen remarked that in one of the tests quoted, the water-dropper had been put out of action by freezing. In this respect at least the radio-active collector would have the advantage. The author's experience as to the relative advantage of the two methods would be of value. Though not arising strictly out of the Paper the question occurred as to whether the earth as a whole was charged, or whether the electric fields observed extended only between the surface and the upper atmosphere. Was it yet possible to decide this point ?

Author's reply : In a very severe climate freezing is a serious drawback to a water jet collector, and radio-active collectors are there probably much to be preferred. They have been used, for instance, successfully by Dr. G. C. Simpson in Lapland, and in the Antarctic. In temperate climates I think—though others will differ—that the balance of advantages lies with the water-dropper, at least when there is a suitable water supply. The efficiency of the water jet has no tendency to decay with time. It is also very markedly greater than that of any radio-active collector I have come across, an important point when insulation is imperfect. In very calm weather the radio-active collector may infect the immediate neighbourhood. Loss of record from freezing of the jet is a very rare occurrence at Kew. As a source of trouble it is not to be mentioned in comparison with spider webs or inferior insulation in damp weather. The object of exhibiting the trace which was interrupted by the freezing of the jet was to show the leakage due to the imperfect insulation.

Observations of potential gradient made from balloons in fine weather have shown a rapid decline in the gradient with increase of height above the ground. The inference is that the lower layers of the atmosphere contain a charge opposite in sign to that on the earth's surface. Balloon observations have not, so far as I know, ever shown an actual reduction to zero in the potential gradient. But the natural inference from the somewhat limited data available is that under normal conditions the lowest 10 km. of the atmosphere contain a charge opposite in sign to that in the earth's surface, and making at least a close approach to equality with it.

XIII. *A Further Improvement in the Sprengel Pump. By* J. J. Manley, *M.A., Research Fellow, Magdalen College, Oxford.*

Received July 10, 1922.

ABSTRACT.

The Paper relates to a further improvement in the pump recently described in the Proceedings of the Society,* which is designed to avoid irregularities due to air skins on the inner surfaces of the apparatus. The present improvement consists in means for providing a mercury seal during periods when the pump is out of use, whereby the formation of fresh air skins is prevented.

In a Paper* recently communicated to the Physical Society, I described an elaborated form of the Sprengel pump and detailed a plan whereby certain irregularities arising from the presence of air skins upon the interior surfaces of the apparatus may be entirely avoided.

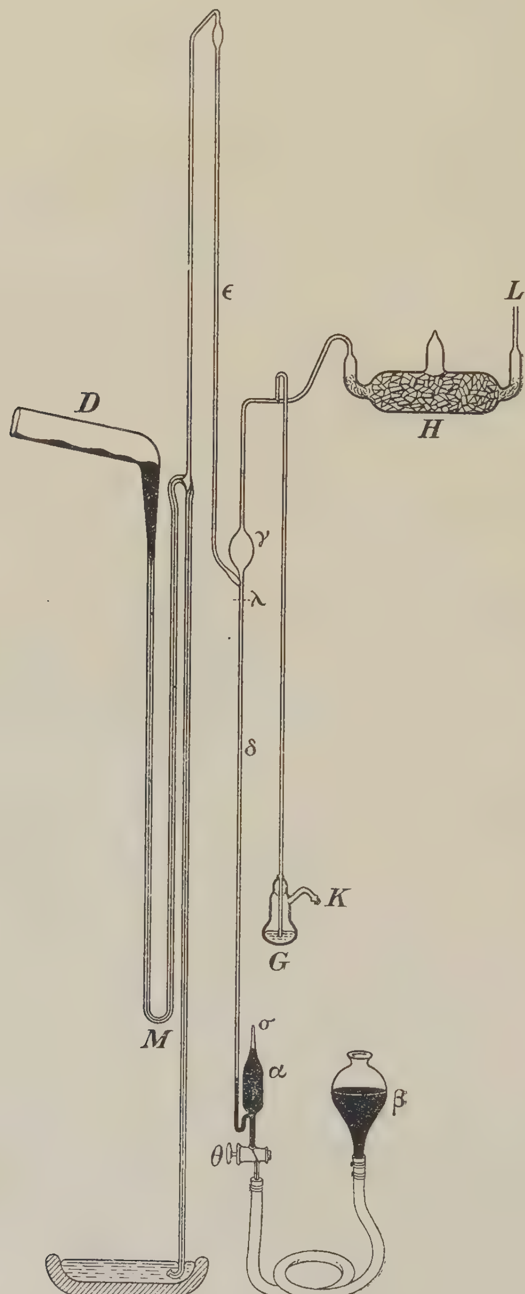
Now although the pump when duly prepared for action functions exceedingly well, a critical examination of the apparatus and its condition leads us to infer that a very appreciable defect is still retained; and the reality and importance of the defect will probably be readily admitted by all who view the matter in the following way.

On readmitting air to the exhausted apparatus, the tube leading to the head of the pump is almost entirely freed from mercury; in consequence of this, the incoming air at once begins to form a skin upon the hitherto gas-free surface. When the pump is again used, the erosive action of the mercury flowing through the tube slowly wears away the newly formed skin and the gas, thus set free, accumulates in the form of small bubbles within the head of the pump from whence it is discharged at irregular intervals into the fall tube; a little of the gas is carried away by the dropping mercury, but the major portion penetrates in the direction of the vessel that is being evacuated; and the more perfect the existing vacuum the greater the ease with which the eroded gas finds its way into the exhausted apparatus. The defect has recently been remedied by interposing between the head of the pump and the apparatus to be exhausted two superposed barometric columns δ and ε, as shown in the accompanying figure. The pump in its newest and more perfect form is prepared and used in the following way:—

The entire apparatus is first made chemically clean and dry; and then for the air skin covering the whole interior surface is substituted an equivalent skin of carbon dioxide, the substitution being effected as already described in my former Paper (*vide supra*). Next, the several reservoirs of the pump, including β, are partially but suitably charged with mercury, and β slowly lifted until the gas within the body of the trap α has been expelled through the as yet open capillary tube σ; the capillary is then sealed by fusing its tip. Temporarily closing the tube L, the two chambers† serving as large air traps are successively evacuated with a Geryk pump; the one is then closed by mercury, but the other, D, is hermetically sealed by fusing the capillary through which the chamber was exhausted. Lastly,

* Proc. Phys. Soc., Vol. 34, Part 3, p. 86.

† The two chambers are fully depicted in the former Paper.



ε
L
D
H
γ
λ
δ
K
G
σ
M
α
β
θ

the Geryk pump is connected with the tube K and the columns δ and ε, together with the chamber H, exhausted until the mercury has ascended in the column δ to a point λ but little below the barometric height. The reservoir β is now suitably raised and the bulb γ almost filled with mercury ; on closing the tap θ, the Sprengel pump is fully prepared for use and any apparatus to be exhausted may be attached to the tube L.

On opening L prior to the attachment of a vessel, the barometric column which is at once set up within ε completely excludes air from the head of the pump ; hence the formation of a new gas skin upon the interior surfaces of the tubes meeting in the head of the pump is rendered impossible. As the primary exhaustion of a vessel by a Geryk pump operating through K proceeds, the barometric column ε continues to shorten and ultimately retains a small value only ; at this stage the tap θ is opened and the cistern β lowered until the mercury in the bulb γ has fallen to the level λ ; free communication is thus established between the Sprengel pump and the partially evacuated vessel ; the required vacuum can then be produced in the usual way.

XIV. *Null Methods of Measurement of Power Factor and Effective Resistance in Alternate Current Circuits by the Quadrant Electrometer.*

By D. Owen, *B.A., D.Sc., F.Inst.P., Sir John Cass Technical Institute, London.*

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ABSTRACT.

Zero methods are proposed, and expressions derived, for the measurement of power factor and effective resistance of alternating current loads. The methods are extended to high tension circuits.

The effect of "electrical control" of the needle of the quadrant electrometer is discussed, and it is shown that the usual formula for the instrument is applicable only when the needle is maintained at its mechanical and electrical zero. The further advantages of null methods are emphasised.

Illustrative tests are recorded.

Introductory.

The measurement of electrical power by the quadrant electrometer has until recently been made exclusively by deflectional methods. When first applied to this purpose the procedure consisted in taking two readings, the second being in the nature of a correction on account of the power consumed in the auxiliary resistance in series with the load. Miles Walker* showed how the latter reading could be avoided by connecting the needle to the mid-point of a non-inductive resistance (or transformer) placed as a shunt across the main leads; the reading is then proportional to the power required. The law of the instrument may under these conditions be written

$$W=K\theta \quad \ldots \ldots \ldots \ldots \quad (1)$$

where W denotes the power in watts, θ the angle of deflection of the needle (a unifilar torsional control being assumed), and K is a multiplying factor to be ascertained by a calibration test on a purely non-inductive load. In practice, for accurate work, it is necessary to calibrate the scale of the instrument at every point. Furthermore if, as is usually the case, the needle experiences a torque depending on its voltage even when the quadrants are at one potential—in other words, if *electric control* is present—the scale value in watts varies according to the root mean square potential of the needle.

If, however, the voltages on needle and quadrants are adjusted so that the deflection of the needle is zero, no calibration is necessary, and the only condition to be satisfied is that

$$\overline{(V_1-V_2)\left(V-\frac{V_1+V_2}{2}\right)}=0 \quad \ldots \ldots \ldots \ldots \quad (2)$$

where V_1 and V_2 denote the potentials of the quadrants and V that of the needle at any instant; the bar over the expression on the left denotes that the mean value is to be taken. The proof of this is given in an appendix to the present Paper.

Two modes of connection of the electrometer to the circuit will be considered, with either of which the power supplied may be deduced when the adjustment of the needle to zero has been made.

* Journ. Am. Inst.E.E., p. 1035 (1902).

Now in a balance method it is clear that this balance should be independent of the voltage across the circuit, except in so far as the power factor is itself dependent on that voltage. It therefore appears that balance methods should lead to the determination of the power factor, and similarly of the effective resistance, without the necessity of knowing either the current or the applied voltage, as would be required when the actual power is measured. It thus seems desirable to regard these methods from this point of view. Indeed, for many purposes the knowledge of power factor, and its variation with conditions of temperature, applied voltage, etc., is of more interest than that of the power itself. It will be shown that null methods do in fact lead to the calculation of both power factor and effective resistance.

The increased accuracy of which they permit, in virtue of non-interference by electrical control, the fact that extreme steadiness of voltage is not to the same extent necessary, and the freedom from laborious calibration, give them a clear advantage over deflectional methods. In addition, they possess the merits of null methods in general—namely, freedom from errors arising from imperfect elasticity of the suspension, and increased sensibility whenever the deflection in the corresponding deflectional method would be off the scale.

The methods referred to may be applied to high-tension circuits, with connections suitable for use with the usual low voltage electrometer.

Methods of Connection and Formulæ.

Method I. (*Double-shunt Connection*).

H. Parry* proposed a mode of connection involving a shunt resistance, which admits of the adjustment of the deflection of the needle to zero, the data at balance

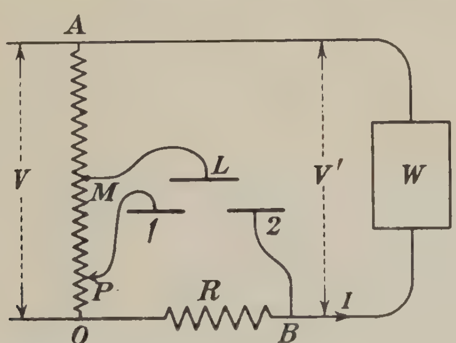

Fig. 1.—Modified Parry Connection.
OA = shunt resistance ; W = load ; L = needle ; quadrants denoted by 1 and 2.

being sufficient to enable the power to the load plus that to the series resistance to be calculated. In the course of the discussion on the Paper it was pointed out that the method could be improved by connecting the needle to the mid-point of the shunt, in which case the term for the power in the series resistance is eliminated. The circuit as modified is shown in Fig. 1. By adjusting the ratio $N = OA/OP$ of the full voltage V across the circuit to that between O (which may

* Proc. Phys. Soc. Lond., p. 217 (1921).

conveniently be regarded as at a constant zero of potential) and the travelling point P, the condition of zero deflection may be secured for any value of R, and the power W is given by

$$W=\frac{V^2}{R}\cdot\left(\frac{1}{N}-\frac{1}{N^2}\right) \quad \ldots\ldots \quad (3)$$

where R denotes the value of the series resistance. The measurement of the power thus requires a reading of the voltage across the mains, as well as knowledge of the shunt-ratio and the series resistance.

To obtain the power factor an auxiliary balance is necessary, the connection of the needle being removed from M to O, and a new shunt ratio N' observed. This balance is simply one of voltages; the voltage V/N' pulling the needle towards quadrants 1, whilst the voltage RI across the series resistance pulls the needle towards quadrants 2. The balance implies equality of these voltages, quite irrespective of their relative phases. Hence we can write

$$V/N'=RI, \text{ or } V/I=\text{impedance of load including } R=N'R \quad \ldots \quad (4)$$

where I denotes the load current.

The two balances supply all the data for calculating the power factor $\cos\varphi$, and the effective resistance R_e (defined by the relation $W=I^2R_e$) as well as the power W. Denoting the voltage across the load (the series resistance being excluded) by V', we may write

$$W=V'I\cos\varphi=I^2R_e=\frac{V^2}{R}\left(\frac{1}{N}-\frac{1}{N^2}\right).$$

Combining with (4) we obtain

$$R_e=R\,.\,(N-1)\,.\,N'^2/N'^2\,. \quad \ldots\ldots \quad (5)$$

and

$$\cos\varphi=\frac{N'^2\,.\,(N-1)/N}{\sqrt{N^2\,(N'^2-1)-2N'^2\,(N-1)}} \quad \ldots\ldots \quad (6)$$

The expression in (6) for the power factor in terms of N and N' is rather cumbrous, and it is perhaps more satisfying to calculate it in steps, with the guidance of the vector diagram, as represented in Fig. 2. Dividing each side by the current I we have Fig. 3, differing only in scale from Fig. 2. As R, R_e, N and N' are the data supplied by the balance, now known, the sides $A'B'$ and $A'D'$ of the right-angled triangle $A'B'D'$ are known, whence $B'D'$ can be calculated; $B'C'$, the load impedance, may now be found, and thence $\cos\varphi(=R_e/\text{impedance of load})$.

When the supply is a high-tension one it is no longer possible, using the ordinary low-voltage type of electrometer, to place this tension (or half of this) on the needle. If, however, the needle connection be made to a point F on the shunt, such that OF is a fraction $1/m$ of the circuit voltage; and if at the same time quadrants 2 are connected across the fraction $R/\frac{1}{2}m$ of the series resistance, as shown in Fig. 4, then balance may be obtained by adjustment of P, the connection to the other pair of quadrants, and the formula (3) applies without alteration. The value of m is to be chosen so that the voltage applied to the needle is near to its normal working voltage. (*See also* Paper by Miles Walker, *loc. cit.*)

As before, a second balance is obtained on transferring the connection of the

needle to O. The formula $V/N'=2\,IR/m$ then applies, differing from that used on low-voltage circuits for the reason that the fraction $2/m$ of the total drop across R is being utilised between the needle and the second pair of quadrants. With this difference the calculation of power factor or effective resistance proceeds as before.

The use of the very high resistance shunts needful on high-tension circuits is,

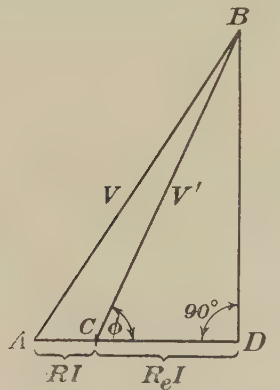

FIG. 2.—VECTOR DIAGRAM OF VOLTAGES.

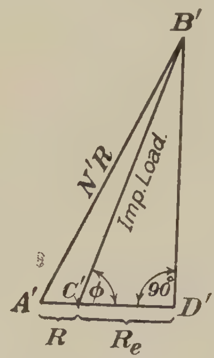

FIG. 3.—VECTOR DIAGRAM OF VOLTAGES WHEN $I=1$ AMP.

however, expensive, as well as undesirable on other grounds. This difficulty has often been met in practice by the use of step-down transformers. In view of the accuracy with which the transformation-ratio and the phase error between primary and secondary voltages of a suitably designed transformer can be determined,*

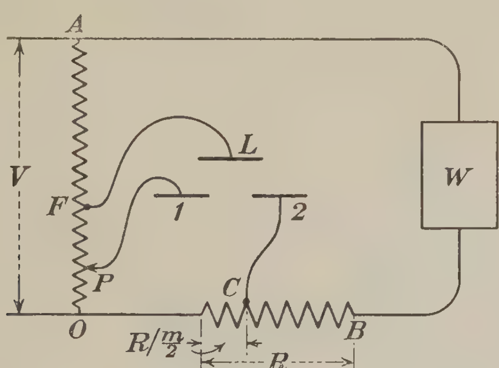

FIG. 4.

their employment appears feasible provided they are used within the prescribed limits of their calibration. Assuming, then, the use of a transformer with step-down ratio m, we may apply the voltage V/m direct to the needle, and obtain balance by connecting the quadrants 1 to the travelling point P on a non-inductive shunt

* Rosa and Lloyd, and Agnew and Fitch, Bull. Bureau of Standards, Washington, Vol. 6 (1909).

placed across the secondary, quadrants 2 being connected to the intermediate point on R as already specified. The diagram of connections is shown in Fig. 5.

To illustrate: Suppose V=20,000 volts, load current I=0·2 ampere, and $\cos\varphi$=0·01. Choosing m as 100, the voltage applied to the needle becomes 200. The resistance R might be taken as 1,000 ohms, making the voltage drop applied

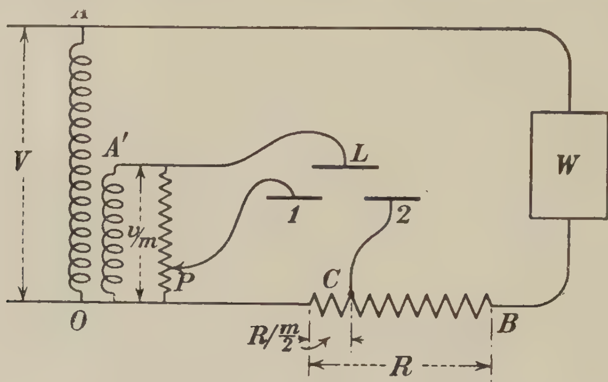

FIG. 5.—USE OF STEP-DOWN TRANSFORMER.

to the quadrants 2 equal to 200/50=4 volts. This would admit of an accuracy of 1/10 per cent. in the power factor.

Method II. (Single-shunt Connection).

This mode of connection, which has been known for some time (*see* Russell, Alt. Curr., Vol. 1, Ch. 9, p. 196), has the appearance of greater simplicity, and, moreover, enables the effective resistance of the load to be determined from a

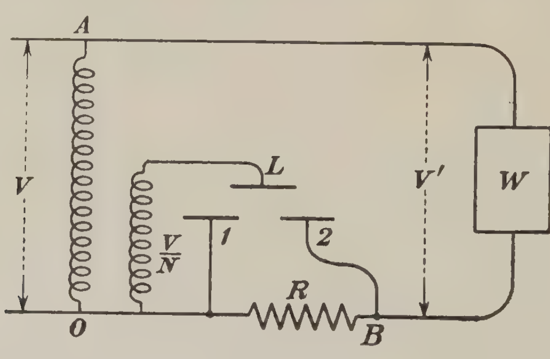

FIG. 6.—SECOND NULL METHOD—THAT OF SINGLE-SHUNT CONNECTION: BALANCE OBTAINED BY ADJUSTMENT OF R.

single balance only. The accuracy obtainable is very satisfactory when the power factor is low, as on condenser loads; owing, however, to a certain lack of symmetry it loses sensitivity as the power factor approaches unity, and the accuracy is not then comparable with that obtainable by the previous method.

On high-tension circuits it has the advantage that the use of a non-inductive shunt may be avoided altogether if the effective resistance only is required. The diagram of connections is then as shown in Fig. 6. Using a step-down transformer of ratio N, the point O is common to one side of the primary, one side of the secondary, and one side of the series resistance. Keeping the voltage on the needle fixed, a balance may be effected by varying the value of R. The condition for balance is expressed in the formulæ

$$W=I^2R_e=I^2R\,.\,(N-2)/2 \quad (7)$$

whence

$$R_e=R\,.\,(N-2)/2 \quad (8)$$

Thus let $V=20{,}000$ volts, $I=0{\cdot}2$ ampere, $\cos\varphi=0{\cdot}01$; then choosing N equal to 100, voltage on the needle is 200 volts, and the value of R required for balance is 0·816 ohms, obtainable by a low resistance shunted by an adjustable high resistance. The accuracy of the determination of effective resistance, which works out at 1,000 ohms, would be about one quarter of 1 per cent.

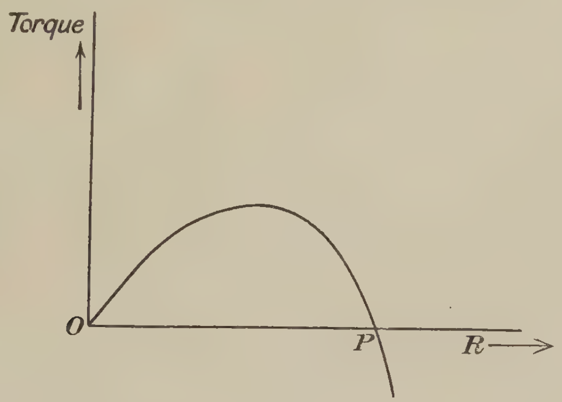

FIG. 7.

To obtain the power factor, a second balance exactly similar to that required in the first method is made, this needing the use of a shunt across the secondary, a tapping from which is connected to quadrants 1, whilst the needle is connected to O. If at balance the fraction $1/N'$ of the full voltage V is applied to quadrants 1, we have

$$RI=V/N' \quad (9)$$

N', which is greater than N, is obtained as the product of N into the tapping ratio of the shunt on secondary. The power factor $\cos\varphi=C'D'/C'B'$ may be calculated, as in the previous method, by reference to the vector diagram of Fig. 3, using the data of balance according to equations (8) and (9).

It should be noted that there are two values of R for balance. The curve connecting the torque on the needle with R is very nearly a parabolic one, as shown in Fig. 7. The balance required is, of course, that corresponding to the point P of the diagram.

Examples of Tests.

The following examples of actual tests are adduced, the method being that of double-shunt connection (*see* Fig. 1) :—

(1) Load consisting of a standard mica condenser, capacity 1 μF, in series with a known non-inductive resistance r. Frequency of supply 96 ~. R=80 ohms.

V	r	N	N'	R_e	cos φ	W	$R_e - r$
(volts)	(ohms)			(ohms)		(watts)	(ohms)
75·5	20·06	754·4	13·89	20·48	0·0184	0·0919	0·42
74·5	10·06	1454	13·98	10·75	0·00964	0·0477	0·69
74·5	2·06	5480	13·90	2·80	0·0025	0·0092	0·74
74·5	0·06	26700	13·90	0·56	0·0005	0·0025	0·50
							Mean=0·60±0·1 ohm

The choice of the above artificial loads of low power factor allows of a severe check on the accuracy of the method. The last column gives the values of the effective resistance of the condenser alone, inferred as the difference between R_e and r. The agreement between them is very satisfactory, and leads to a power factor of 0·0006 for the condenser alone, corresponding to a phase lead of current over voltage amounting to 1′ 56″ short of 90°. This is estimated to be correct within 20″ of arc ; but the sensibility of the electrometer used might have been improved considerably by working with a smaller distance between the upper and lower plates of the quadrants, and this error correspondingly reduced.

(2) Commercial paraffin condenser, capacity 1·09 μF. At 96 ~. R=80 ohms.

V	N	N'	R_e	cos φ	W
74·5	2857	12·93	4·59	0·00445	0·0238

(3) Small transformer, closed iron circuit, on open secondary. At 96 ~. R=110 ohms

V	V'	N	N'	R_e	p.f.	φ
76·0	74·0	38·64	20·30	1143	0·526	58° 15′
38·0	36·9	37·02	14·39	598	0·390	

Though the values of voltage V are given in the above tables, it should be remembered that this is an unnecessary datum so far as the calculation of effective resistance or power factor is concerned.

The author desires to express his obligations to Mr. F. I. G. Rawlins, who assisted in the tests recorded in the Paper, and to Mr. G. L. Addenbrooke, who kindly placed one of his electrometers at his disposal.

Appendix.

The Formula for the Quadrant Electrometer.

Though it has been known for some time, it is not generally realised that in addition to the mechanical controlling couple on the needle there is an electrical control, of magnitude depending on the voltage on the needle. (Reference may be made to a clear exposition of this, due to Dr. R. Beattie, in *The Electrician*, p. 729,

1910.) The following derivation of the formula may be of service as showing the origin of the term for electrical control, as well as the fact that the existence of finite electrical control, though affecting the constant to be used in deflectional measurements, does not enter into the conditions which hold when a balance, *i.e.*, zero deflection of the needle, is attained.

Let C denote the capacity between two conductors differing in potential by V. The mutual torque is $\frac{1}{2}V^2 . dC/d\theta$, where θ denotes the appropriate angle. Applying this to the quadrant electrometer, if C_1 and C_2 denote the respective capacities between a pair of quadrants and the needle, and V_1, V_2, and V the potentials of quadrants and needle, we have

$$\text{torque on needle} = \tfrac{1}{2}(V-V_1)^2 . dC_1/d\theta - \tfrac{1}{2}(V-V_2)^2 . dC_2/d\theta \quad . \quad . \quad (1')$$

Writing
$$C_1 = a_1 + b_1\theta + c_1\theta^2 + \quad . \quad . \quad .$$
$$C_2 = a_2 + b_2\theta + c_2\theta^2 + \quad . \quad . \quad .$$

the expression for the torque becomes

$$\text{torque on needle} = \tfrac{1}{2}(V-V_1)^2(b_1+2c_1\theta+ \ldots) - \tfrac{1}{2}(V-V_2)^2(b_2+2c_2\theta+\ldots) \quad . \quad (2')$$

If $V_1 = V_2 = V$, the needle comes to rest at the "mechanical zero." If

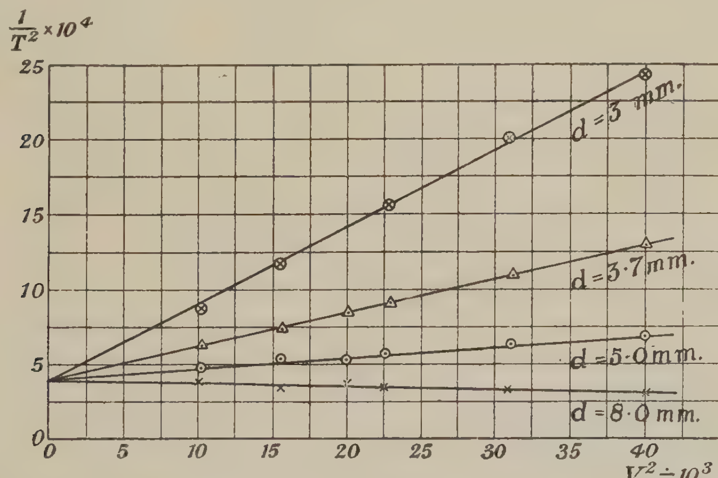

FIG. 1′.—EFFECT OF ELECTRICAL CONTROL ON PERIOD OF NEEDLE.
V = voltage on needle ; T = period in secs.

$V_1 = V_2 = 0$, but $V \neq 0$, the rest position of the needle is the "electrical zero." When the instrument is adjusted for coincidence of these zeros (as is necessary in all modes of use of the instrument), the effect, as may be seen by inspection of (2′), is to make $b_1 = b_2 = b$, say.

The derivation of the usual Maxwell formula involves the assumption that the capacities are linear functions of θ, in which case the electrical control vanishes. In general this simple assumption is unjustified ; agreement with observation may, however, be secured to a close degree by taking the capacities as quadratic functions of θ. Supposing a unifilar suspension to be employed, (2′) thus becomes

$$-k\theta = \tfrac{1}{2}b[(V-V_1)^2 - (V-V_2)^2] + c_1(V-V_1)^2\theta - c_2(V-V_2)^2\theta \ldots$$

Since V is generally large compared with V_1 or V_2, this may, to a near approximation, be written

$$-k\theta=b[(V-V_1)^2-(V-V_2)^2]+(c_1-c_2)V^2\theta,$$

or

$$[k+(c_1-c_2)V^2]\theta=b(V_1-V_2)\left(V-\frac{V_1+V_2}{2}\right) \quad . \quad . \quad . \quad . \quad (3')$$

The controlling torque, proportional to θ, is thus made up of two terms, one with constant k representing the mechanical control; the other with constant (c_1-c_2) representing the electrical control, which may either assist or oppose the mechanical control. The expression for the deflection may be written

$$\theta=\frac{K(V_1-V_2)\left(V-\frac{V_1+V_2}{2}\right)}{1+AV^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4')$$

where K and A are constants; the sign of A may be positive or negative.

On alternating current circuits this becomes

$$\theta=\frac{K\,.\,\overline{(V_1-V_2)[V-\frac{1}{2}(V_1+V_2)]}}{1+A\overline{V^2}} \quad . \quad . \quad . \quad . \quad . \quad (5')$$

In place of the constant K of the simple formula it is necessary to use a coefficient $\frac{K}{1+A\overline{V^2}}$ which involves the root-mean-square value of V.

But in the null use of the instrument $\theta=0$, and the condition for this is

$$\overline{(V_1-V_2)[V-\frac{1}{2}(V_1+V_2)]}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6')$$

which involves a knowledge neither of K nor of A. No calibration of the instrument is required, nor is the presence of electrical control any source of error.

It may be of interest to add a graph (Fig. 1′) showing the magnitude of the electrical control as found in an actual instrument (of the Addenbrooke pattern), for various distances between the upper and lower plates of the quadrants, and for various voltages on the needle. The simplest means of measuring the control is to observe the period of vibration of the needle, with the quadrants at the same potential, for any chosen potential of the needle.

The graphs, which connect the reciprocal of square of the periodic time and the square of the needle voltage, prove to be straight lines, in accordance with the formula (3′). For a distance d of 6·8 mm. between the plates the electrical control vanishes, the period being independent of the voltage on the needle; for smaller distances the electrical control is positive, and for greater distances negative. The electrical control may prove to be actually greater than the mechanical control; it only vanishes for a particular value of the sensibility of the instrument.

DISCUSSION.

Dr. E. H. Rayner congratulated the author on his valuable additions to the many uses of the Quadrant Electrometer. This wonderful instrument was invented over half a century ago by Lord Kelvin, but is still unsurpassed in its utility, being applicable to the accurate measurement of power, insulation, phase-angles, and many other quantities. The speaker

took the opportunity to point out some details as to which care is necessary in the practical use of the Electrometer. (1) With high voltages the mechanical force on the needle is considerable and may bend it, leading to inconsistent results at low power factors. (2) Referring to Fig. 1 of the Paper, the high resistance AO generally has an appreciable distributed capacity, with the result that the voltage across MO is not in phase with the current. If conditions permit, the easiest remedy is to take as much current along OA as possible; for instance, if the current in this branch be 1/20 ampere, the power factor in a common case would be 0·1 or 0·2 per cent., but on increasing the current to 1 ampere the phase lag might become negligible. A similar error has to be contended with where a step-down transformer is used, as shown in Fig. 5, and it must be remembered that for small phase-angles an error of a few minutes of arc may represent a large percentage error. (3) An extremely important point when high voltages are applied to the needle is that the faces of the quadrants should be perfectly flat. To this end they should be ground on cast iron after they have been fixed in place.

Mr. G. L. ADDENBROOKE referred to his Papers published in the *Electrician* in 1901 as relevant to some of the points raised by the author. He added that it is convenient to arrange a switch whereby the point P, Fig. 1 of the Paper, may be connected at will to the point O. In this way the instrument may be converted into an ammeter. He had used deflectional methods because they permitted " seeing what was going on."

Dr. A. RUSSELL congratulated the author on discovering so many theorems and applying them so usefully, and expressed appreciation of Dr. Rayner's helpful suggestions.

Capt. R. DUNSHEATH (communicated): This Paper is very opportune at the present time when so many investigators are seeking the best method of measuring dielectric losses, and is full of useful suggestions. I do not agree with the author, however, that it is desirable to eliminate both voltmeter and ammeter. His methods give power factor only, but a figure for actual watts lost is generally required. Also, due to the importance of the dependence of power factor and losses on voltage, it is usual to decide at the commencement of a test what voltage shall be adopted, and a voltmeter is essential. The ammeter is not so necessary as, having V, N', and R, in formula (4), the value of the current follows at once. Proceeding in this manner $\cos \varphi$ is obtainable without the use of equation (6). It is, of course, necessary to switch one side of the voltmeter from O to B, but this is a simple matter.

I notice that Dr. Owen estimates the error of the figure obtained for phase angle on a $1 \mu F$ mica condenser at about 16 per cent. Much smaller condensers than this are usual in certain branches of industrial work, and it would be interesting to know the sensitivity of the instrument used, and what sort of accuracy might be expected if the method were applied to capacities of the order of $0{\cdot}01 \mu F$.

Mr. HUBERT PARRY (communicated): The most interesting part of Dr. Owen's Paper is, I think, the extension of the null method to other ratios of line voltage to needle voltage than 2. I think it is a very valuable step. I would point out that when taking the needle voltage as some fraction of the line voltage, if the resistance of the potential divider has a large value per volt the capacity of the electrometer and leads may cause the voltage on the needle not to be in phase with the line voltage, and this phase displacement enters directly into the result obtained. When the needle is at line voltage the potential divider accuracy for phase is not so important.

I do not think Method II. as accurate as Method I., as the phase angle of variable series resistances is somewhat uncertain. This is especially so if the resistance value is low. This also applies to shunting one resistance by another. I think this will be realised when working at about 90° phase difference, where one or two minutes' error means a large percentage difference in the result.

I doubt if the phase error of a potential transformer would be sufficiently constant for precision measurement of, say, condenser losses; a potential divider on the lines of Örlich and Schultz would be better, and there probably would not be such a lot of difference in the cost.

In practice $\cos \varphi$ in equation (6) could be expressed by watts/VI without serious error, provided the voltage drop on the series resistance is not large compared to the needle voltage.

I do not think it is quite correct to say that " the presence of electrical control is no source of error " in the zero method; if the voltage on the needle varies the zero varies, and this will lead to incorrect balancing.

Mr. A. ROSEN (communicated): A great advantage of balance methods which measure power factor directly is that $\cos \varphi$ varies approximately as V^{n-2}, when the power W varies as V^n, so that fluctuations in voltage are of less importance, and the voltmeter need not be so accu-

rately calibrated. The figure given for the accuracy when testing at 20,000 volts—viz., 1/10 per cent. of cos $\varphi=0{\cdot}01$—is no doubt deduced from tests taken at low voltages, and appears somewhat optimistic. When working to such a high degree of accuracy, factors enter which might otherwise be ignored—*e.g.*, has the author considered the effect of the time-constants of the resistances in the various parts of the circuit ? Another difficulty is the effect of speed. Although frequency is not mentioned explicitly in any of the equations, it enters as follows : On a condenser load, the impedance is approximately $1/\omega c$ ($\omega=2\pi\times$frequency, $c=$capacity$)=N'R$ from (4). $\therefore$ N' varies approximately as $1/\omega$ since c is constant. Assuming for the condenser that cos φ is roughly constant with frequency, we have from (6) N varies approximately as $1/\omega$. Thus, to measure cos φ to 1/10 per cent., the speed must be controlled and measured to within this figure, an ideal not obtained in practice with the comparatively large machines needed for testing cable at high voltages. Possibly speed variation accounts for the difference of 3 per cent. of 0·01 in the figures quoted for test (1). If so, it is obvious that increasing the voltage or the sensitivity of the electrometer will not, in this case, produce greater accuracy. However, 1 per cent. is sufficient for practical work, and is better than can be obtained with certainty using the wattmeter in the usual way.

The AUTHOR (in reply to the discussion) : The remarks of Dr. Rayner will be valued by practical workers. The question of phase error in the shunt resistance, and methods of compensation have also been treated by Örlich and Schultze. Mr. Addenbrooke's desire to follow what was going on by watching the deflection was, of course, quite natural. In the present methods this could always be done in the preliminary tests by slipping P into coincidence with O (*see* Fig. 1) ; for the final reading the zero balance would confer a distinct gain in accuracy. In reply to Capt. Dunsheath's inquiry the sensitivity of the electrometer used in the tests quoted was such that with 100 volts on the needle and one-tenth of a volt across the quadrants the deflection was about 60 mm. at a metre scale-distance. This could have been multiplied three or four times possibly. Measurements on a 0·01 μF condenser could be conducted with much the same accuracy as those with $1\mu F$, since the resistance R could be increased in inverse proportion to the capacity.

In reference to Mr. Parry's last remark, the zero of needle should not vary as the voltage on needle varies ; nor have I found that it does in my own experiments. Mr. Rosen's observations in regard to effect of frequency are of value, and show the necessity of securing constancy of speed of machines in proportion to the accuracy aimed at.

XV. *The Eötvös Torsion Balance.*

By Captain H. Shaw, *M.Sc., F.Inst.P., and* E. Lancaster-Jones, *B.A.* (*Cantab*).

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ABSTRACT.

The Eötvös Gravity Balance in the Science Museum, South Kensington, is described, and the theory of its operation considered. A full account is given of certain preliminary experiments made on the torsion wires, with a view to ascertaining the daily variation of the equilibrium position, due to strains set up in the wires during manufacture. Until this daily shift has been eliminated the instrument cannot be employed successfully. The experiments show that a preliminary treatment of the wires is desirable, and a test was also made of the " baking " method recommended by Eötvös for ageing the wires. This method was found to give the desired result, and after this treatment the reading of the instrument remained practically constant from day to day.

Tables are given showing the variation in the reading of the instrument when its beam is rotated into different azimuths, and the constancy of these readings when repeated. The differences in the readings as the azimuth varies are sufficiently marked to indicate the changes in the local gravitational field of the laboratory, and further experiments are being carried out to determine these changes in detail, as a preliminary to field tests ; it is hoped to communicate the results of these experiments at a later date.

Previous to the introduction of this instrument, researches on gravity were confined almost exclusively to investigations with the pendulum and the bubble level, but in 1888 a new line was followed by Eötvös, who endeavoured to measure the variation of the force of gravity in the vicinity of a point, or more exactly, to determine the derivatives of its components, while analogous methods were also employed to determine the derivatives of the magnetic force.

As the variations of gravity are extremely small in comparison with the total force, Eötvös concluded that the method to be employed should measure the differences of gravity directly, rather than the force of gravity itself.

This method, it is claimed, is extremely sensitive, and is capable of giving results which are unobtainable by pendulum methods. According to Eötvös, it is possible by this method to estimate the desired magnitudes at any place in the course of one night, with an accuracy of 1×10^{-9} C.G.S. units, while under favourable weather conditions the accuracy may be increased still further, but it is unfavourably affected by rapid variations of temperature.

In the construction of an instrument sufficiently sensitive for this purpose, the main characteristic required is a long period of swing, and it is due to this consideration that an instrument has been produced, capable of observing and measuring these small variations of force.

The balance was designed in 1888 by Baron Roland Eötvös, Professor of Physics at the University of Budapest, and was constructed by the firm of Ferdinand Süss. It is intended to be used as a field instrument, and is therefore designed with a view to portability, while at the same time it is capable of determining variations of the above-mentioned order, with a considerable degree of accuracy.

It consists of a fine torsion wire, carrying a lever which supports at its extremities two weights, at different vertical heights, the whole being enclosed in a double-walled metal case which can be rotated about a vertical axis. An azimuth circle

enables the positions of the case to be determined and the orientation of the balance arm relative to it is observed by the aid of a telescope. The system has a period of swing exceeding 1,200 seconds, and after having been disturbed returns to rest in its equilibrium position in approximately two hours. The position of equilibrium and the motion of the beam are found to be remarkably stable and relatively constant, so that the instrument can be used not only in a well-protected laboratory, but also at night in the open air, with the protection only of a canvas tent.

(A.) Theory.

The beam is freely suspended under the action of two force-systems, viz. :—

(*a*) The force of gravity, which is compounded of forces due to earth-attraction and forces due to earth-rotation.

(*b*) Torsional forces due to twisting of torsion-wire.

As regards the system (*a*) we make the following assumptions :—

1. The complete system has a potential function U, which is *uniform* in the neighbourhood of any point external to the earth's masses, and of which the first derivatives $\frac{\partial U}{\partial x}, \frac{\partial U}{\partial y}, \frac{\partial U}{\partial z}$, and the second derivatives $\frac{\partial^2 U}{\partial x^2}, \frac{\partial^2 U}{\partial x \partial y}, \frac{\partial^2 U}{\partial y^2}, \frac{\partial^2 U}{\partial z^2}$, &c., are also uniform at such points for any system of rectangular axes.

2. The masses of the torsion balance beam are so arranged that it has only a tendency to twist about the torsion wire.

As regards (*b*) we assume :—

3. The force due to twist is proportional to the angle through which the beam is twisted from its position of zero torque. The zero position is usually unknown until the series of observations is complete.

Choose rectangular axes of co-ordinates (x, y, z), with the origin O at the centre of gravity of the balance beam, the axis Oz directed vertically downwards, the axis Ox towards the geographical north and Oy towards the east.

Let (x, y, z) be any point in the suspended system.

Let (X, Y, Z) be the forces at (x, y, z) per unit mass due to system (*a*) resolved along the axes.

Then, since a potential function U is assumed to exist for these forces, we have

$$X=\frac{\partial U}{\partial x}, \quad Y=\frac{\partial U}{\partial y}, \quad Z=\frac{\partial U}{\partial z}.$$

At the origin O, we have $X_0=Y_0=O$, and $Z_0=g_0$, since the resultant force is assumed to be along the Oz axis at O.

At any other point (x, y, z) we have, expanding by McLaurin's Theorem,

$$\left.\begin{aligned} X&=X_0+x\left(\frac{\partial X}{\partial x}\right)_0+y\left(\frac{\partial X}{\partial y}\right)_0+z\left(\frac{\partial X}{\partial z}\right)_0+\text{ higher orders} \\ Y&=Y_0+x\left(\frac{\partial Y}{\partial x}\right)_0+y\left(\frac{\partial Y}{\partial y}\right)_0+z\left(\frac{\partial Y}{\partial y}\right)_0+\text{ higher orders} \\ Z&=Z_0+x\left(\frac{\partial Z}{\partial x}\right)_0+y\left(\frac{\partial Z}{\partial y}\right)_0+z\left(\frac{\partial Z}{\partial z}\right)_0+\text{ higher orders} \end{aligned}\right\} \quad \ldots \ldots \quad (1)$$

If we assume that, in the region covered by the balance, the forces X, Y, Z are so nearly uniform that we can neglect terms involving x^2, y^2, &c., and the second derivatives of X, Y, Z, we have

$$\left.\begin{aligned} X &= x\left(\frac{\partial X}{\partial x}\right)_0 + y\left(\frac{\partial X}{\partial y}\right)_0 + z\left(\frac{\partial X}{\partial z}\right)_0 \\ &= x\left(\frac{\partial^2 U}{\partial x^2}\right)_0 + y\left(\frac{\partial^2 U}{\partial x \partial y}\right)_0 + z\left(\frac{\partial^2 U}{\partial x \partial z}\right)_0 \\ Y &= x\left(\frac{\partial^2 U}{\partial x \partial y}\right)_0 + y\left(\frac{\partial^2 U}{\partial y^2}\right)_0 + z\left(\frac{\partial^2 U}{\partial y \partial z}\right)_0 \end{aligned}\right\} \text{since } X_0 = Y_0 = O. \quad . \quad . \quad . \quad (2)$$

The torque F_a due to system (a) about axis Oz will be

$$F_a = \int (Yx - Xy)\, dm,$$

where the integral is taken over the whole suspended mass system.

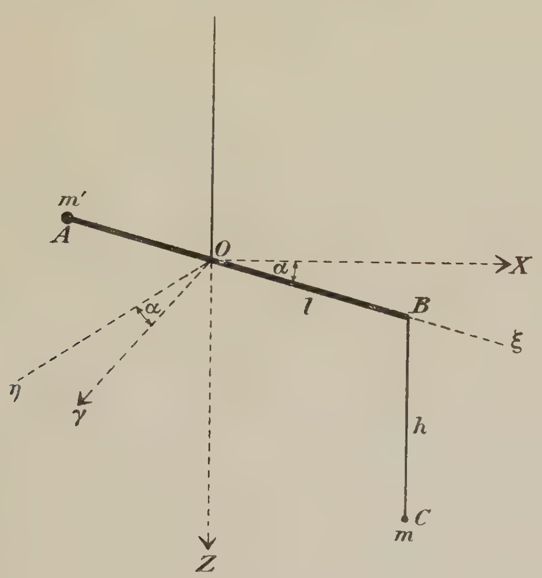

FIG. 1.

Using equations (2), we get

$$F_a = \left(\frac{\partial^2 U}{\partial y^2} - \frac{\partial^2 U}{\partial x^2}\right)_0 \int xy \,.\, dm + \left(\frac{\partial^2 U}{\partial x \partial y}\right)_0 \int (x^2 - y^2) \,.\, dm + \left(\frac{\partial^2 U}{\partial y \partial z}\right)_0 \int zx\, dm$$
$$-\left(\frac{\partial^2 U}{\partial x \partial z}\right)_0 \int yz dm \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Let the beam when in equilibrium be inclined at an azimuth angle α to axis Ox, and take new axes O (ξ, η, z), such that $O\xi$ lies along the axis of figure of the beam, $O\eta$ perpendicular thereto (both in the horizontal plane xOy).

Then, if (ξ, η, z) denote the co-ords. corresponding to (x, y, z) for every point in the suspended mass, we have

$$x=\xi\cos\alpha-\eta\sin\alpha$$
$$y=\xi\sin\alpha+\eta\cos\alpha$$

$$\therefore \quad \int xydm=\tfrac{1}{2}\sin 2\alpha\int(\xi^2-\eta^2)\,.\,dm+\cos 2\alpha\int\xi\eta\,.\,dm$$

$$\int(x^2-y^2)dm=\cos 2\alpha\int(\xi^2-\eta^2)\,.\,dm-2\sin 2\alpha\int\xi\eta\,.\,dm$$

$$\int xz\,.\,dm=\cos\alpha\int\xi z\,.\,dm-\sin\alpha\int\eta z\,.\,dm$$

$$\int yzdm=\sin\alpha\int\xi z\,.\,dm+\cos\alpha\int\eta z\,.\,dm$$

By symmetry about axis $O\xi$ and plane ξOz, we have

$$\int\xi\eta\,.\,dm=O,$$

$$\int\eta z\,.\,dm=O,$$

$$\int\xi z\,.\,dm \text{ (for } m' \text{ and beam)}=O,$$

$$\int\xi z\,.\,dm \text{ (for } m \text{ alone)}=mhl,$$

where l = distance OB from origin to point of suspension of lower weight.
h= distance BC from pt. of suspension of lower weight to its centre of gravity

Also we may put $\int(\xi^2-\eta^2)\,dm=K'$

Although K' does not equal the moment of inertia K of the whole system about Oz, it is usually assumed to do so, since η^2 is negligible in comparison with ξ^2.

$\therefore$ on this understanding we have for equation (3)

$$F_\alpha=\frac{K}{2}\,.\,\sin 2\alpha\left(\frac{\partial^2U}{\partial y^2}-\frac{\partial^2U}{\partial x^2}\right)+K\cos 2\alpha\left(\frac{\partial^2U}{\partial x\partial y}\right)+mhl\left[\frac{\partial^2U}{\partial y\partial z}\cos\alpha-\frac{\partial^2U}{\partial x\partial z}\sin\alpha\right]\,. \quad (4)$$

where the suffix [*i.e.* $(\ )_0$] has been dropped, as there is no further ambiguity and the quantities $\left(\frac{\partial^2U}{\partial y^2}-\frac{\partial^2U}{\partial x^2}\right)$, $\frac{\partial^2U}{\partial x\partial y}$, &c., are understood to have their values at the origin O.

Now, since the beam is in equilibrium under the torque F and the resistance to twist, which latter is equal to $\tau\theta_\alpha$, where θ_α is the twist in the wire for the position α, we have

$$F_\alpha=\tau\theta_\alpha.$$

Let n_α = the scale reading (n scale units of 0·5 cm.) corresponding to the α position of equilibrium.
,, n = scale reading when there is zero torque (*i.e.*, $F=O$).
,, D' = distance (in scale units) of mirror from scale.

Then $n_a - n = 2D'\theta_a$.

Equation (4) becomes

$$n_a - n = \frac{2D'K}{\tau}\left[\left(\frac{\partial^2 U}{\partial y^2} - \frac{\partial^2 U}{\partial x^2}\right)\frac{\sin 2a}{2} + \frac{\partial^2 U}{\partial x \partial y}\cos 2a\right] + \frac{2D'mhl}{\tau}\left[\frac{\partial^2 U}{\partial y \partial z}\cos a - \frac{\partial^2 U}{\partial z \partial x}\sin a\right] \tag{5}$$

putting

$$\frac{D'K}{\tau}\left(\frac{\partial^2 U}{\partial y^2} - \frac{\partial^2 U}{\partial x^2}\right) = A$$

$$\frac{2D'K}{\tau} \cdot \frac{\partial^2 U}{\partial x \partial y} = B$$

$$-\frac{2D'mhl}{\tau} \cdot \frac{\partial^2 U}{\partial z \partial x} = C$$

$$\frac{2D'mhl}{\tau} \cdot \frac{\partial^2 U}{\partial y \partial z} = D$$

we have

$$n_a - n = A \sin 2a + B \cos 2a + C \sin a + D \cos a \quad \ldots \ldots \tag{6}$$

(B.) Determination of Constants.

In the equation (6) the quantities A, B, C, D and n are unknown factors, independent of the azimuth angle a of the beam. By taking readings of n_a in 5 different positions of a, these factors can be evaluated from the resultant 5 equations.

The factors $\frac{2D'K}{\tau}$ and $\frac{2D'mhl}{\tau}$ are purely instrumental constants, independent of the position of the balance, and are evaluated previous to the observations in the following way.

To find τ.—An accurately turned lead sphere to attract the suspended weight m is placed alternately on the left and right side of m, in a line perpendicular to the direction of the beam and at a measured distance from m as small as possible (about 10 cm.). Then if

$n' - n$ = displacement of zero at a setting of lead sphere
D' = distance of scale from mirror
G = gravity constant
M = mass of lead sphere
m = ,, suspended cylinder
λ = length of suspended cylinder
ρ = mean distance of centre of sphere from axis of cylinder
l = distance OB

we have

$$\tau \cdot \frac{n - n'}{2D'} = G\frac{Mm}{\rho^2} \cdot \frac{l}{\sqrt{1 + \frac{\lambda^2}{4\rho^2}}} \quad \ldots \ldots \tag{7}$$

neglecting effect of sphere on mass m'.

To find K.—The quantity $\frac{K}{\tau}$ can be found by ascertaining the period of swing of the system in two positions of the balance arm, $\alpha=0$, and $\alpha=\frac{\pi}{2}$. Owing to the long period, however, it is preferable to substitute a stiffer and shorter wire, and to find the periods of swing T_0 and T_1, firstly for the original suspended mass system (m', m), and secondly when extra weights m_1, m_2 are suspended on the beam. The moment of inertia of the added weights can readily be calculated, and the unknown coefficient of torsion of the wire can thus be eliminated.

Thus, if the new wire has a coefficient of torsion τ', we have for the first oscillation, with only m and m' suspended on the beam,

$$\frac{K}{\tau'}=\frac{T_0^2}{4\pi^2}$$

where T_0 secs. represents the complete undamped period of torsional oscillation, neglecting all effects due to varying gravity field owing to the unimportance of these when the wire is sufficiently rigid. This could be tested by oscillating in different azimuths.

For the second oscillation, with added weights m, on each side, and a resultant added moment of inertia I,

$$\frac{K+I}{\tau'}=\frac{T_1^2}{4\pi_2^2}$$

$$\therefore \quad \frac{K+I}{K}=\frac{T_1^2}{T_0^2}$$

$$\frac{I}{K}=\frac{T_1^2-T_0^2}{T_0^2}. \qquad (8)$$

which gives K in terms of I.

By means of the above preliminary experiments, and ordinary measurements and weighings for D', m, h, l, we find the instrumental constants $\frac{2D'K}{\tau}$ and $\frac{2D'mhl}{\tau}$. Hence, by observing n_α in five positions of the beam and thus obtaining A, B, C and D we can get the four magnitudes

$$\left(\frac{\partial^2 U}{\partial y^2}-\frac{\partial^2 U}{\partial x^2}\right), \quad \left(\frac{\partial^2 U}{\partial x \partial y}\right), \quad \left(\frac{\partial^2 U}{\partial y \partial z}\right), \quad \left(\frac{\partial^2 U}{\partial z \partial x}\right) \text{ at the origin } O.$$

(C.) Numerical Values.

To show the magnitude of the quantities concerned, one may give the values in C.G.S. units, of the above quantities for the normal Bessel Ellipsoid, where the semi-axes of the ellipsoid a, b, are

$$a=637{,}739{,}700 \text{ cm.}$$

$$b=635{,}607{,}800 \text{ cm}$$

and by Helmert's formula for g,

$$g=978{\cdot}00(1+0{\cdot}00531 \sin^2\varphi).$$

For latitude $\varphi=51°30'$ (London) we get

$$g=981{\cdot}1806$$

$$\frac{\partial g}{\partial x}=\frac{\partial^2 U}{\partial z \partial x}=7{\cdot}9376\times10^{-9}$$

$$\frac{\partial g}{\partial y}=\frac{\partial^2 U}{\partial y \partial z}=0$$

$$\frac{\partial^2 U}{\partial x \partial y}=0$$

$$\left(\frac{\partial^2 U}{\partial y^2}-\frac{\partial^2 U}{\partial x^2}\right)=4{\cdot}005\times10^{-9}$$

In one of Eötvös's instruments, the instrumental constants $\frac{2D'K}{\tau}$ and $\frac{2D'mhl}{\tau}$ amounted to $0{\cdot}10323\times10^9$ and $0{\cdot}14087\times10^9$ respectively. If this instrument were set at $\alpha=\frac{\pi}{2}$ at a place where gravity was "normal" in latitude 51°30′, the equation would be

$$n_\alpha-n=-(4{\cdot}005\times0{\cdot}05162)-(0{\cdot}14087\times7{\cdot}9376)=-1{\cdot}3 \text{ approx.},$$

whilst at $\alpha=0$, we should have

$$n_\alpha-n=0$$

$\therefore$ the reading at $\alpha=\frac{\pi}{2}$ would differ from that at $\alpha=0$ by approximately 1·3 scale divisions, each division being 0·5 mm. This would be easily appreciable. Where, however, there are great local attracting masses, the differences in readings as α varies often amount to ten times the above quantity. For example, in a room of the Physical Institute of the University of Budapest, Eötvös got the following readings for different azimuths, of the beam.

α	n
0°	204·5 [scale unit=0·5 mm.]
72°	200·7
144°	193·2
216°	183·2
288°	199·1

The corresponding values of $\frac{\partial^2 U}{\partial x \partial z}$, &c., being

$$\frac{\partial^2 U}{\partial x \partial z}=+21{\cdot}12\times10^{-9}$$

$$\frac{\partial^2 U}{\partial y \partial z}=-66{\cdot}89\times10^{-9}$$

$$\frac{\partial^2 U}{\partial x \partial y}=+10{\cdot}25\times10^{-9}$$

$$\left(\frac{\partial^2 U}{\partial y^2}-\frac{\partial^2 U}{\partial x^2}\right)=+66{\cdot}08\times10^{-9}$$

(D.) Description of the Instrument.

Torsion Wire.

The balance arm G is supported by a 60 c.m. length of fine platinum-iridium wire B of 0·04 mm. diameter, and containing 20 per cent. of iridium. Special precautions are taken to prevent deformation of the wire, which is supplied by the makers on a reel of not less than 25 cm. diameter. The ends of the wire are soldered to brass eye-plates which serve to attach them to the beam, and the torsion head A. The wire, which is capable of supporting a weight of 120-130 gr. is stretched under a load of 80 gr. which is approximately the weight which it has to carry in the instrument, after which it is subjected to a special baking treatment, which consists in gradually raising its temperature to 100°C. in an oven, and allowing it to cool slowly. A wire treated in this manner appears to lose the greater portion of its remanent torsion, and according to Eötvös may be used the following day, although it still exhibits a slow displacement of the equilibrium position which amounts to a few minutes of arc per day, but is sufficiently regular to enable its value to be calculated and a correction applied. This variation gradually decreases, but apparently is only eliminated after several years.

A far greater disadvantage arises from the varying susceptibility to temperature effects possessed by the torsion wires according to the permanent twist set up during manufacture. The position of equilibrium thus varies with the temperature, but after baking, this variation is found not to exceed a few tenth minutes per degree centigrade, and can be corrected by the use of an estimated temperature, coefficient.

Quartz fibres were tried by Eötvös subsequently, but were found by him to be not so sensitive as platinum, as he found great difficulty in getting a quartz fibre capable of supporting 100 grm. and having a torsion coefficient per metre length, of less than unity, whereas that for his platinum wire was 0·3 C.G.S. units. According to Eötvös the constancy of the equilibrium position of similar well-drawn platinum wires was found to be so satisfactory that he preferred them, especially in portable field apparatus, to the quartz fibres, which are too delicate and fragile.

Torsion Head.

The upper end of the torsion wire is attached to a torsion head A which enables the wire and beam to be adjusted, while the central member to which the torsion wire is attached is threaded through an annular collar, which is itself threaded into the body of the torsion head, thereby enabling vertical adjustment of the wire and beam to be secured. The torsion head is graduated on its circumference in intervals of 5 deg., a vernier enabling readings to be taken to degrees. A three-screw clamping device is also provided, by which the torsion head may be adjusted and clamped in position

Beam.

A light aluminium rod forms the balance arm G, which is rectangular in section and 40 cm. in length. From a V-notch near one end of this beam a cylindrical platinum weight K is suspended by a fine platinum wire J. This cylinder, which weighs about 25·5 grm., has a length of 27 mm. and a diameter of 8 mm. and is suspended with its centre of gravity about 65 cm. below the beam. To the other end of the beam a 30 grm. counterweight D is riveted, consisting of a platinum

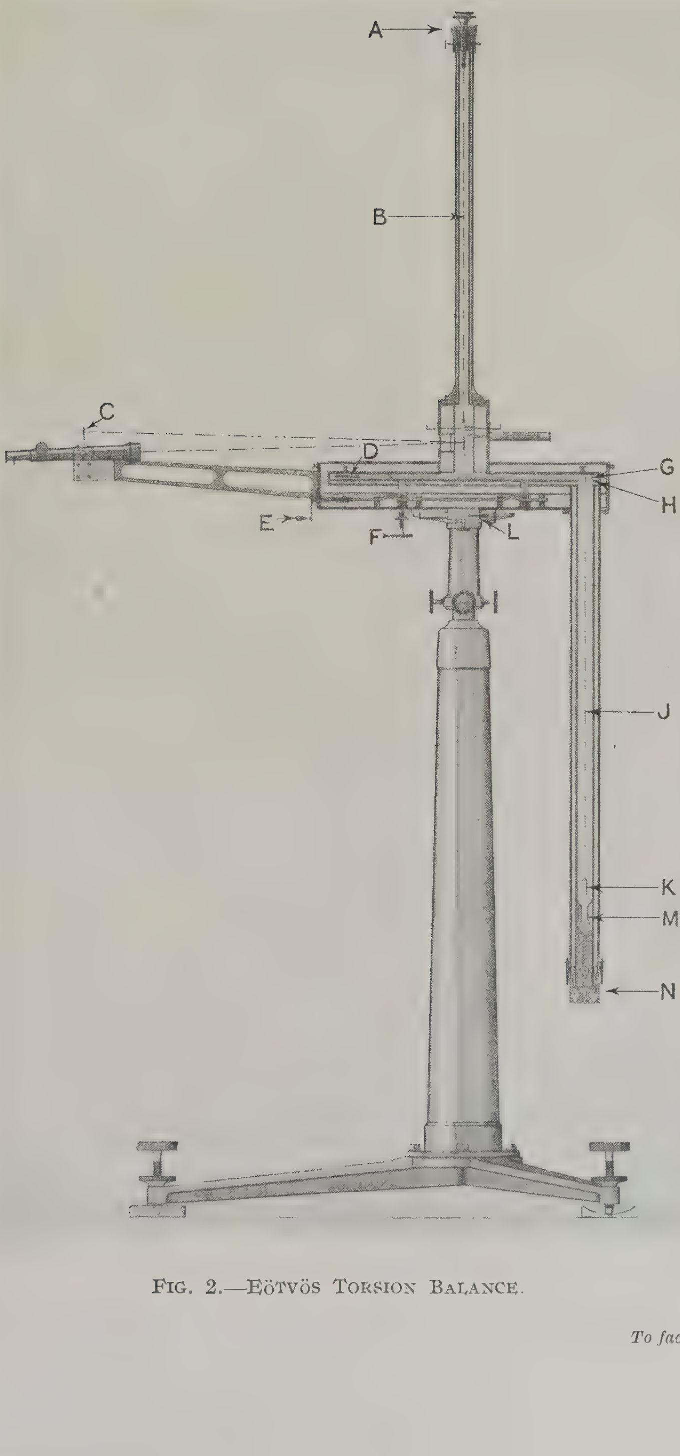

FIG. 2.—EÖTVÖS TORSION BALANCE.

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strip 85 mm. by 12 mm. bent in the form of a U, and fixed with its maximum dimension in the direction of the axis of the beam.

A light vertical rod 10 cm. long rises from the centre of gravity of the beam, and serves to carry a small circular mirror, while a small screw near its extremity facilitates the attachment of the torsion wire.

Case.

The sensitiveness of the instrument is such that special protection is required against all external influences which are able to affect in any way its operation and equilibrium position. It is possible to realise those conditions by using double-walled metallic boxes which, without interfering with the motion of the bar, enclose and protect it as much as possible. The boxes are of brass, with walls 3 mm. and 4 mm. thick respectively and separated by an air gap of 1 cm. It is claimed that with this construction temperature changes penetrate uniformly into the interior of the instrument and consequently do not give rise to air currents while the metallic envelope, homogeneous on all sides, also furnishes protection against electrical action and radiations of all kinds. The degree of stability obtained in this manner is such that the instrument is capable of furnishing satisfactory results in the field, protected only by a canvas tent. The effect of solar radiation, however, is a factor of importance, and the best results are obtained at night time or on cloudy days.

In this instrument the horizontal balance beam *G* is enclosed in a double-walled rectangular box, the inner and outer covers of which are easily removable, thereby exposing the beam. A double-walled box enclosing the mirror and its support is split centrally into two portions which are easily removable, giving ready access to the mirror and the lower end of the torsion wire. In one of these portions of the box a glass window is provided, enabling the reflection of the scale *C* in the mirror to be read by the telescope.

Screwed to the upper surface of the mirror box a double-walled vertical tube terminating in a torsion head provides protection for the torsion wire, while a similar tube projecting downwards from one end of the beam case protects the lower weight and its suspension.

The inner box enclosing the beam has a movable base *H* which is capable of adjustment vertically by means of a handle *E* at one end of the case. In this way the base of the box can be lowered or raised for damping the motion of the beam, and also forms a platform upon which the weight of the beam can be taken, thus releasing the tension of the torsion wire for the purpose of transport. The beam is also clamped in position by means of two forked arms projecting horizontally into the interior of the mirror case, into which they may be pushed so as to clamp the aluminium support of the mirror.

A clamping device is also provided for the suspended weight *K* in the form of a clamp and rest *M* carried on a vertical screw working in a fixed collar at the lower end of the enclosing tube and operated by a screw cap *N*. Rotation of this cap in one direction causes the rest to be raised sufficiently to enclose the platinum weight and to raise it slightly, thus removing the tension from the suspension. The platinum weight is automatically clamped in this position by a vertical wire carried on the rest, and operating in a vertical groove in the inner tube. A projection on

the end of this wire moves inwards above the platinum weight when the latter has been taken up by the rest, and so holds it in position, should the instrument be overturned during transport.

The instrument is read by a telescope carried on an arm hinged to the case, and also carrying above the telescope a scale graduated in half-millimetres, at a distance of 60 cm. from the mirror. The telescope arm is adjusted by means of a screw F bearing on the under surface of the beam case, while the telescope itself may be adjusted by means of two set screws so that the graduated scale is visible in the mirror. By this means the scales can be read to 0·05 mm., which is the equivalent of 8·6 secs. of arc.

Mounting.

The case and telescope can be rotated about a vertical axis mounted on a fixed horizontal stage carrying a circle graduated in thirds of a degree and provided with a vernier L, by means of which the azimuth of the case can be read to an angle of one minute. The instrument stands on a heavy tripod base, to which is bolted a vertical centre column, the upper end of which terminates in a cylindrical rod of smaller diameter, and upon which the upper portion of the instrument is carried. Four flats on the exterior of this rod enable four adjusting screws on the upper part of the instrument to be employed for levelling purposes, after the centre column has been adjusted vertically.

A later type of instrument is provided with two suspended beams arranged side by side, so that a complete set of readings can be made by taking three observations, inclined at angles of 120 deg. In this way the period required for the observations at any place is reduced very considerably, enabling the determination to be made in one night.

An automatic device has also been introduced for recording the reading photographically, and turning the instrument into the required azimuth positions at suitable intervals. The balance can then be set up during the day, and left to make its own observations during the night, so that it may be transported and erected at the next station the following day.

The practical use of the instrument lies in its ability to locate large subterranean masses, which differ considerably in density from their surroundings, and it is understood to be in use by a number of oil companies in various parts of the world for the location of salt domes.

(E.) Preliminary Tests of the Instrument.

A fundamental assumption made in the theory of the balance is that for a constant gravitational torque, the beam takes up a fixed equilibrium position, in which the torque is balanced by the torsion of the suspending wire. It is, however, well known that for some time after their manufacture, thin torsion wires are subject to elastic " after effects," owing to the gradual recovery of the wires from the twist they sustain in being drawn. The consequence of this is that the equilibrium position of the beam gradually changes even when the remainder of the instrument is fixed.

The first test made therefore was to determine this " shifting of the equilibrium position " from day to day, firstly in the case of a torsion wire used just as it was

received from the makers, and, secondly, in the case of a wire which had been heated repeatedly to 100°C., and allowed to cool before being used, this being the treatment employed by Eötvös to render his wires stable. The results of this preliminary test are represented graphically in Fig. 3, which gives the day to day shift of the equilibrium position for two wires *A* and *D*, during the periods indicated. The wire *A* was not subjected to any treatment before use, and was observed for a period of two months, the instrument remaining undisturbed throughout. The wire *D* was specially treated by being slowly heated under a load of 80 gm. to 100°C. in an oven, and gradually cooled, the operation being repeated 50 times during a period of 10

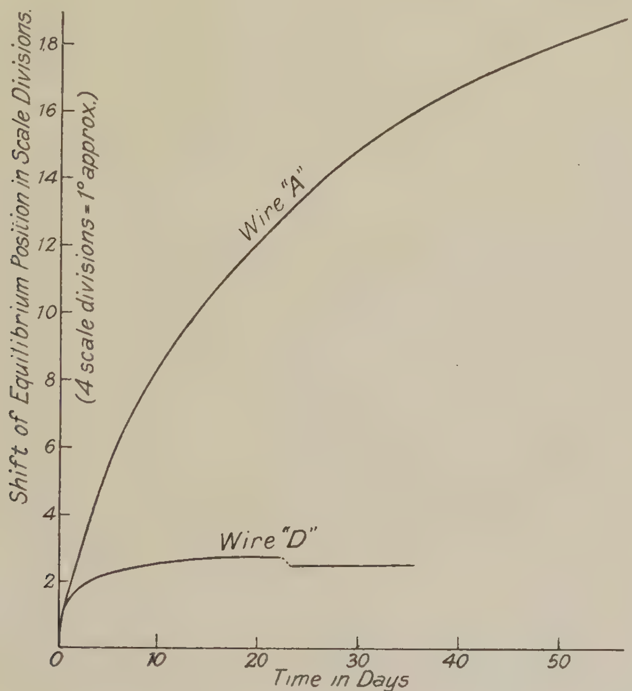

FIG. 3.

days, after which it was transferred to the balance and its behaviour observed for about 40 days.

From the above test the advantage of treating the wire is obvious and it is apparent that in all cases the wire must be subjected to some previous treatment in order to stabilise it, if results are to be obtainable within a reasonable period of time. The method of treatment here adopted may be regarded as fairly satisfactory, although several days must elapse before the wire reaches a serviceable stage.

SECOND TEST.

The next test was intended to establish the fact that, for a fixed position of the instrument, the equilibrium position of the beam varies in different azimuths, but

is constant for any given azimuth. The following tables give the scale readings corresponding to the six azimuth positions, 0°, 60°, 120°, 180°, 240°, 300°, the upper portion of the instrument being rotated through 60° in a clockwise direction after each observation, and allowed to come to rest in its new position.

TABLE I.—*For "D" Wire.*

0°.	60°.	120°.	180°.	240°.	300°.	Date.
21·14 21·13 21·13 21·13	20·17 20·15 20·17 20·15	21·85 21·83 21·88 21·83	22·38 22·35 22·37 22·30	19·91 18·89 19·96 19·89	20·30 20·29 20·34 20·30	May 28 to June 14.
21·13	20·16	21·85	22·35	19·91	20·31	Mean.

TABLE II.—*For "D" Wire.*

0°.	60°.	120°.	180°.	240°.	300°.	Date.
21·50 21·50 21·51	20·52 20·51 20·51	22·22 22·24 22·23	22·74 22·75 22·75	20·27 20·28 20·28	20·69 20·69 20·70	July 12-17.
21·50	20·51	22·23	22·75	20·28	20·69	Mean.

The differences evident from a comparison of corresponding columns of these tables, are probably due in the main to the liability of the wire, even after having been stable for several weeks, to sudden "shifts," after which it again takes up a stable position; another contributory factor was the removal of a number of heavy objects from one adjoining room to another during the intervening period.

The results indicated in Table II. agree more closely than those in Table I., and this may be ascribed to the increased age of the wire, the observations being taken a month later. These tables also give an indication of the sensitivity of the balance and the magnitude of the differences between the various deflections, which with a properly stabilised torsion wire enable results to be obtained to within approximately 1 per cent., as claimed by Eötvös.

The instrument on which the tests were made is the property of the Science Museum, South Kensington, the experiments being conducted in the basement of that institution, and our thanks are due to the Director, Col. H. G. Lyons, F.R.S., who has afforded us every facility in this work.

The conditions of the test as regards constancy of temperature were very favourable, but the reverse was true as regards liability to shocks and variation in neighbouring masses. Notwithstanding this, the reading of the instrument remained very stable.

Further experiments are being undertaken to determine the constants of the instrument, and to investigate the variations of the gravitational force within the laboratory in order to gain experience with the instrument under laboratory conditions, before employing it in the field.

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DISCUSSION.

Col. H. G. Lyons said that it was some ten years since, as Director of the Science Museum, he first endeavoured to obtain an example of this instrument, but the execution of his plan had been delayed by the war. The authors had been working in unfavourable circumstances owing to the disturbing effects of alterations which were being carried on at the Museum, so that it was very creditable to have achieved consistent results. It was important to know whether in their experience the instrument is sufficiently shielded from the effect of temperature variations, as it is very desirable that it should be used in field observations. Prof. Soler claims to have obtained results in the field which showed a marked difference in the readings at stations quite close together, the readings being repeated consistently on a subsequent survey. The balance is proving of use commercially, for instance, in connexion with the survey of oil fields in Egypt. The oil companies, unfortunately, appear to have been under the impression that to obtain the requisite calculations it was necessary to send the observations to Germany !

Sir Gerald Lennox-Cunningham said he was astonished to see such a sensitive instrument working so satisfactorily in the demonstration given by the authors. It might well have been expected that without a special floor and special means of protection irregular results would be obtained. He inquired as to the damping arrangements adopted.

Dr. C. Chree said that he first became acquainted with the instrument twenty years ago, when he read a German account of it which struck him as very obscure and difficult to follow. The present treatment in English will be of great value. The fact that it has remained practically unknown in this country for so long seems to show the need of a Geophysical Institute, for at present it is no one's business to keep the scientific world informed of progress in this branch of physics. Col. Lyons has done a public service in securing a trial of the balance, which may have the greatest value industrially.

Mr. J. Guild referred to the assertion that the balance can measure the difference between the values of g at places a decimetre apart. As the dimensions of the instrument itself are of this order, it is not quite clear how this claim is substantiated.

Mr. F. E. Smith also commented on the extreme accuracy claimed for the balance which amounts to an error of only one part in a million million. The accurate determination of gravitational acceleration was undoubtedly a difficult problem. He recalled how the value of g was measured by an Antarctic expedition at Melbourne, then at Antarctic stations, and subsequently at Melbourne again. The values at Melbourne were found to be different on the two occasions, and this difference was attributed to a real change in g; but it might have been due to instrumental errors. Is there any record in the Continental literature of a comparable set of observations ? He agreed as to the need for a Geophysical Institute. The measurement of gravity had made little progress in this country since the time of Kater.

Dr. E. H. Rayner said that it was important to consider elastic fatigue of the suspension in an instrument of this kind. In his own experience of electrometers elastic fatigue appeared to be due mainly to the attachment at the two ends of the suspension wire, for a wire giving perfectly consistent results will show fatigue if owing to any accident it has to be resoldered. It would be valuable if authors in this and any accurate work involving fine torsion wires could

record the effect of altering the fixing of the wire ; such differences as those shown in Fig. 3 of the Paper might be explicable on these lines. Tungsten wires can now be obtained, and it might be an improvement to substitute them for the platinum-iridium wires used.

Mr. L. F. Richardson recalled that tungsten wire, at least that available in 1914, showed a tendency to break up into irregular strands.

Capt. H. Shaw gave an account of some improved forms of the balance. The improvements have been mainly directed to the elimination of errors due to asymmetry of the instrument and to enabling observations to be taken more rapidly. A double instrument is used so that the six readings can be taken in the time previously required for three, and a photographic recorder makes it possible to determine the static deflection with greater certainty. With these improvements it has been found possible in Germany to deal with three stations a day.

Mr. E. Lancaster-Jones, replying to the discussion, said that the damping could be regulated by raising or lowering the bottom plate of the balance casing and so varying the air space between it and the beam ; but it was found impracticable to bring the beam to rest in less than 1½ hours. Temperature effects are not troublesome, a change of a few degrees producing results small compared with those due to the g-gradient. Whereas five observations completely determine the gradient, it is usual to take six so that a check may be obtained, and the results are consistent to 1 per cent. Field observations are taken at night to avoid the sun's heat, and no more protection is required than that afforded by a canvas tent. As regards the claim to accuracy, what is deduced is the gradient of g, assumed to be uniform in a given locality, and this gradient multiplied by a decimetre gives a quantity greater than the probable error. What is actually measured, however, is the integral of the effect of the gradient over a length of about 60 cm.

XVI. *The Crystalline Structure of Anthracene.* *By* Sir W. H. Bragg, *F.R.S.*

Received December 21, 1922.

ABSTRACT.

The author in an address recently published* put forward evidence for regarding the benzene ring as an actual structure of ascertainable size and form, and deduced that the unit cells of naphthalene and anthracene should have two of their axes equal, the third axis being longer for anthracene than for naphthalene by the width of one ring. The experimental data then available supported this hypothesis only roughly; in the present Paper it is shown that more reliable data, subsequently obtained from small but perfect anthracene crystals, agree with it very closely. It is pointed out that the X-ray data furnish a new and accurate method of determining the density of a crystal.

In an Address which as President I gave to this Society on November 11th, 1921, I described certain experiments on the structure of organic crystals. Among these were measurements on the dimensions of the crystal units of naphthalene and anthracene. The comparison of the two sets of results was used to show that the molecule in each case lay with its longest dimension in the direction of the *c* axis. The anthracene data were not entirely satisfactory, since only one X-ray measurement could be made, and that not very accurately: this was the spacing of the cleavage plane 001. The dimensions of the unit were calculated also from the crystallographic data given by Groth and from a determination of the specific gravity quoted in Kaye and Laby's tables. The results were, however, sufficiently accurate to show that the major length of the molecule lay along the *c* axis, because the difference between the lengths of the *c* axes of naphthalene and anthracene was 2·9, while the width of the benzene ring as found in the diamond was 2·5. It was assumed that the molecule of anthracene might be expected to be longer than the molecule of naphthalene by the width of the extra ring, and that the small difference between the two values did not affect the argument.

Recently, through the kindness of Dr. Brady, I have had the opportunity of measuring the anthracene constants, using some minute but perfect crystals which he gave me. The results are set out below in a form which allows comparison with the naphthalene constants. It will be seen that there is now much better agreement between results as calculated and as expected. The *c* axis of anthracene is really 11·18, not 11·6 as calculated from the data previously available. Using, therefore, the X-ray data alone, we find that the difference between the *c* axes of the two crystals is 2·5 exactly, which was the figure anticipated.

The quite considerable difference between the new and the old results is due for the most part to the value previously adopted for the specific gravity. Now that the constants of the crystal unit are more accurately known, it is possible to calculate the specific gravity of a perfect crystal. It is to be expected that this will always be higher than the specific gravity found by ordinary means. It is, in fact, usual for crystallographers to make several measurements of specific gravity and to accept the highest as the most correct (Tutton, "Crystallography," new edition, page 625). The value of the specific gravity is now shown to be 1·255.

There is remarkable similarity between the naphthalene and anthracene observations. Not only are the crystals isomorphous, but the same planes are in each case the

* Proc. Phys. Soc., Vol. 34, Dec. (1921).

best reflectors, and the spacings of the 100 and the 010 are almost exactly the same; it is only in the length of the c axis that there is any marked difference. The cell contains two molecules in each case, while the symmetry number is 4, that is to say, if the molecule were unsymmetrical it would be necessary to employ four of them to make up a cell having the symmetry of the monoclinic prismatic class to which the crystals belong. As two molecules are able to give fourfold symmetry, each must have a twofold symmetry. The nature of that symmetry is indicated by the results. In both cases the 010 plane is exactly halved. Also in the 010 zone, that is in the case of the planes passing through the b axis, 100 and $10\bar{1}$ are halved, 001 and $20\bar{1}$ are not halved. These results show that the crystal molecule possesses a centre of symmetry, and that if one set of molecules all alike to each other be placed at the corners of the cell, the centres of the other molecules lie at the centres of the ab face.

ANTHRACENE.

Monoclinic prismatic. Two molecules in the crystal unit. Assume :—

$$a=8{\cdot}58\ ;\ b=6{\cdot}02\ ;\ c=11{\cdot}18\ ;\ \beta=125°0'$$

	Assumed.	*Observed.*	
100	7·02	3·51	Strong
010	6·02	3·01	Moderate
001	9·16	9·16	Strong. Also 2nd & 4th orders, 3rd order weak.
	Calculated.	*Observed.*	
$10\bar{1}$	8·34	4·16	
$20\bar{1}$	4·17	4·16	Moderate
110	4·58	4·57	Very strong
$11\bar{1}$	4·88	4·90	Weak
$21\bar{1}$	3·43	3·43	Strong
210	3·04	3·04	Strong

Axial ratios in accordance with the above are

$$a : b : c=1{\cdot}423 : 1 : 1{\cdot}857,\ \beta=125°0'$$

Groth gives (Chemische Krystallographie V., p. 437):

$$a : b : c=1{\cdot}4220 : 1 : 1{\cdot}8781,\ \beta=124°24'$$

Specific gravity (calculated from the X-ray data) $=1{\cdot}255$
Specific gravity (redetermined by using Dr. Brady's crystal) $=1{\cdot}250$.

NAPHTHALENE.

(*See* Structure of Organic Crystals, Proc. Phys. Soc., 34, December 15, 1921.)
Monoclinic prismatic. Two molecules in the unit cell.

$$a=8{\cdot}34\ ;\ b=5{\cdot}98\ ;\ c=8{\cdot}68\ ;\ \beta=122°44'$$

These values are in better agreement with the experimental results described in the Paper referred to than the values calculated from the crystallographic data; the differences are small, however.

DISCUSSION.

Dr. D. OWEN inquired whether the measurements show identical dimensions for all the unit cells of a given substance, whatever the specimen taken ; or whether the results vary with the state of strain of a crystal, which might be expected to affect its density.

Dr. E. A. OWEN said that metallurgists would agree with the remark made by the author in presenting his Paper, that X-ray measurements were more dependable than direct measurements of the density of a crystal. In alloys particularly the densities of the component crystals would be difficult to find.

Mr. J. GUILD said he was interested in the statement that by X-ray measurements it was possible to determine the density of a crystalline substance despite the presence of impurities which would vitiate ordinary density measurements. Would it be possible in a matrix of two or more crystalline substances to determine the density of each ? For example, could the density of the cementite in a pearlitic steel be determined ?

Dr. H. BORNS said that in neutralising H_2SO_4 with lime in organic work chemists often obtain fine crystals of various shapes which after all turn out to be impure $CaSO_4$. Have such and similar crystals been examined as to the identity of their unit cells ?

Dr. F. L. HOPWOOD said that the structure of mixed crystals has been investigated by Vegard,* who showed that the lattice of Thorite (Th SiO_4), which belongs to the Zircon group, was completely broken down, only the outer form being preserved.

AUTHOR'S reply : Such evidence as is available supports the idea that the size and form of the crystal unit cell is always the same to a high degree of accuracy, except, of course, that strain or expansion of the crystal is shared by the cell. Each kind of crystal in a matrix gives its own spectrum quite independently. For instance, if cementite crystals occurred in sufficient quantity in a powdered mass the Debye-Hull photograph would show the spectrum of cementite with others due to other crystals. If the cementite lines could be disentangled and interpreted, and if the chemical composition is known, the density is found.

The author is not aware of any systematic measurement of $CaSO_4$ crystals, but does not doubt that they all have the same unit cell no matter what the outside form may be.

* Phil. Mag. (1916).

XVII. *On the Frequency of Vibration of Circular Diaphragms. By* J. H. POWELL, *M.Sc., F.Inst.P., and* J. H. T. ROBERTS, *D.Sc.*

RECEIVED JANUARY 24, 1923.
(COMMUNICATED BY SIR ERNEST RUTHERFORD F.R.S.)

ABSTRACT.

The Paper describes measurements of the natural frequency of diaphragms of various sizes having a rigid rim and a central boss for attachment of a microphone or receiver. In air the frequency was found by means of a monochord, but under water the resonance-frequency was noted in the neighbourhood of a subaqueous transmitter of variable pitch. The results are in good agreement with the theoretical conclusions of Prof. H. Lamb.* In many cases harmonics were observed round about a fifth or an octave above the fundamental, but their occurrence was capricious, and their pitch inconsistent with theory. The resonance peaks of the frequency curves are more or less of the same area, being high and narrow or low and broad.

The effect of increasing the pressure on one side of the diaphragm was studied, and the pressure-displacement curve was found to be linear up to the elastic limit, while the pressure-frequency curve is of the saturation type. A large diaphragm is less affected by pressure (and therefore by immersion in deep water) than a small one of the same natural frequency, in consequence of its greater thickness.

PART I.

THE object of the research described in the following Paper has been to make an experimental investigation of the theory of vibration of circular diaphragms and also to study the vibrations of such diaphragms under various conditions.

In a recent publication,* Prof. H. Lamb has given an account of a mathematical investigation of the frequency of vibration of thin circular diaphragms rigidly clamped at their circumference ; he has considered the vibration of these diaphragms under various conditions in air and water and has deduced certain expressions by means of which the frequency of a diaphragm of any given material can be readily calculated.

The diaphragms used in our experiments were turned out of solid ingots of the metal (steel or bronze) with a rim $\frac{1}{2}$ in. broad round the circumference, and with a small boss at the centre for the attachment of a detector.

It will be seen therefore that the conditions are only approximately those assumed in Prof. Lamb's calculations, but it was shown that variations in the rigidity of the clamping of diaphragms with a rim of the dimensions used did not produce a variation of more than $\frac{1}{4}$ per cent. in the frequency. The boss at the centre of the diaphragm also produced a variation in the frequency for which a correction had to be applied before the results could be compared. For ease in calculation of this correction the boss was kept of the same size in all the diaphragms considered, with the exception of the $6\frac{1}{2}$ in. diameter diaphragms, which were of a special type. The essential parts of this type of diaphragm are shown to scale in Fig. 1, and it will be observed that it was not necessary to attach this diaphragm to any holder while making the observations, as it was itself sufficiently massive. The smaller diaphragms were, for testing purposes, clamped down rigidly to a massive steel holder, illustrated in Fig. 2.

* Proc. Roy. Soc., A., Vol. 98, p. 205 (1920).

Experimental Methods.

A very accurate measurement of the frequency of vibration of a diaphragm in air was possible by simply striking the diaphragm with a soft rubber hammer and tuning a monochord to the same pitch. In order to test the accuracy of this

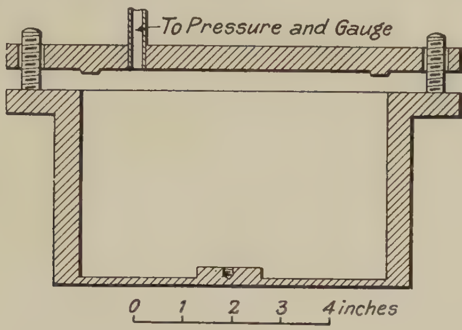


Fig. 1.—Standard $6\frac{1}{2}$in. Diaphragm with Cover Plate. (Cover raised slightly.)

method, the diaphragms were excited electrically by a small electro-magnet, through which an alternating current of controllable frequency was passed.

It was found that in every case these two methods gave values for the frequency which did not differ by more than 0·5 per cent. in the most extreme case.

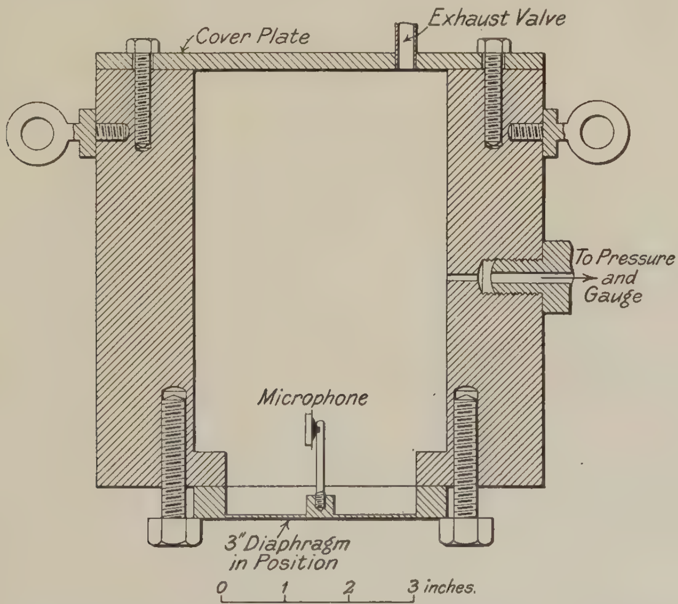


Fig. 2.—Diaphragm Holder Fitted for Pressure Experiments.

In water the method was not so simple, for the damping produced by the water made it much more difficult to estimate the pitch of the note emitted by the diaphragm. The method adopted was therefore to lower the diaphragm in its holder

into water, in a tank constructed for the purpose, so that the diaphragm was immersed with one side in contact with the water. It was then excited by the vibrations transmitted through the water from a telephone sounder also immersed in the tank. It was found that the resonance frequency of a diaphragm in water was independent of the depth of immersion provided the depth was not less than a few millimetres, and provided the depth was not great enough for the pressure of the water to produce an appreciable distortion of the diaphragm. As the total depth of the water in the tank was about two feet, this difficulty did not arise, and for the purposes of these experiments measurements were always taken at a depth of from 6 in. to 10 in.

The vibrations of the diaphragm were examined by the use of a Brown microphone or telephone attached to the boss at the centre of the diaphragm, as shown in Fig. 2. The microphone acts as a small load on the diaphragm, but the telephone, the spring reed only being attached to the diaphragm, was found not to act as a load on the latter.

A simple Dolezelak* alternator was used to excite the telephone sounder in the tank, and by measuring the speed of rotation an accuracy of 0·1 per cent. was obtainable in the frequency measurements.

In all cases where microphones or telephone receivers or sounders were used, resonance points always occurred due to their natural frequencies, which were superposed on those of the diaphragm. These, however, were eliminated by comparing the resonance points using different sounders and detectors with the same diaphragm, when the diaphragm resonance points were easily identified.

In Air.—Prof. Lamb has shown that for a rigidly clamped uniform circular diaphragm of any given material vibrating in air the frequency (n) is given by the expression

$$n=\frac{p}{2\pi}=0{\cdot}4745\;\frac{hc_1}{a^2}$$

where h=thickness of the diaphragm, a=radius of the diaphragm, and where c is the velocity of propagation of extensional waves in an infinite thin plate of the same material and thickness and is equal to

$$\sqrt{\frac{E}{(1-\sigma^2)\,\rho_1}}$$

For an iron plate $E=2\times10^{12}$, $\rho_1=7{\cdot}8$, $\sigma=0{\cdot}28$ c.g.s. units, and we have $c_1=5{\cdot}27\times10^5$ cm. per sec.

Thence
$$\frac{p}{2\pi}=2{\cdot}50\,\frac{h}{a^2}\times10^5 \qquad (1)$$

In Water.—The effect of water is virtually to increase the inertia in the ratio $(1+\beta)$, where $\beta=0{\cdot}6689\,\frac{\rho}{\rho_1}\cdot\frac{a}{h}$, ρ being the density of water.

The frequency of a diaphragm with water on one side only, becomes therefore

$$\frac{p}{2\pi}=\frac{0{\cdot}4745}{\sqrt{1+\beta}}\;\frac{hc_1}{a^2} \qquad (2)$$

With water on both sides, the value of β is doubled.

* F. Dolezelak Zeits. Instrumentenk., XXIII., 240 (1903).

Correction for Load.

If a small load on is added to the centre of a diaphragm its effect is to increase the kinetic energy of the plate. In the Paper previously referred to, Lamb gives the kinetic energy of an unloaded plate as being equal to that of a mass $M/5$, concentrated at the centre, where M is the total mass of the diaphragm. Consequently a small additional mass m at the centre is equivalent to a uniformly distributed load of $5m$. The kinetic energy is therefore increased in the ratio $\left(1+\frac{5m}{M}\right)$.

The frequency of a loaded diaphragm in air is therefore

$$\frac{p}{2\pi}=\frac{0{\cdot}4745}{\sqrt{1+\frac{5m}{M}}}\cdot\frac{hc_1}{a^2} \quad \ldots\ldots\ldots\ldots \quad (3)$$

and with water on one side

$$\frac{p}{2\pi}=\frac{0{\cdot}4745}{\sqrt{1+\beta+\frac{5m}{M}}}\cdot\frac{hc_1}{a^2},\ \text{where } m \text{ is small} \quad \ldots \quad (4)$$

Expressing $(1+\beta)$ as a load on the diaphragm it follows that the quantity of water moving with the diaphragm is equal in volume to a hemisphere whose diametrical plane is the surface of the diaphragm.

The experimental conditions therefore may be assumed not to differ materially from the theoretical in which an infinite medium is postulated.

Experimental Results.

1. *Diaphragms in Air.*—The results obtained by the method already described may be summarised in the following table :—

Table I.—*Steel Diaphragms.*

Diameter of plate in inches.	Thickness of plate in mm.	Observed values.	Calculated values.		
		Frequency loaded in air. (Measured.)	Frequency unloaded in air. (Calculated.)	Frequency loaded.	Load in grams
3	1·0	1,416 ∾	1,722 ∾	1,420 ∾	4·3
3	1·5*	2,520	2,580	2,580	Zero.
4	1·5	1,350	1,450	1,330	4·3
4	2·0	1,810	1,940	1,790	4·3
6½	3·0	953	1,100	942	36·3
6½	4·0	1,330	1,475	1,310	36·3
6½	4·5	1,500	1,650	1,500	40·0
6½	6·0	1,922	2,195	1,895	67·0
7	5·0	1,464	1,585	1,450	36·3
7	6·0	1,730	1,900	1,766	36·3
7	6·6	1,902	2,095	1,958	36·3

* This diaphragm was soldered to its rim and was not turned out of a solid ingot.

A large number of other diaphragms were also examined, including many of intermediate size—but the results were in general within the limits of accuracy indicated in the typical results quoted in the above table.

Irregularities were sometimes found in individual diaphragms, but these were undoubtedly due to alteration in the elastic constants of the material due to stresses set up during the turning process.

(2) *Diaphragms with One Side in Water and the Other Side in Air.*

A similar series of measurements was made with the same diaphragms supported in their holders below the surface of the water in the tank. The following table gives the results obtained for typical diaphragms :—

TABLE II.

Diameter of Plate in inches.	"a" Radius in centimetres.	Thickness of Plate in millimetres.	Frequency with water on one side.			Load, in grams.
			Observed.	Calculated.		
			Loaded.	Unloaded.	Loaded.	
3	3·815	1·0	788	825	770	4·3
3		1·5	1,348	1,448	1,358	4·3
4	5·08	1·5	...	675	658	4·3
4		2·0	956	1,018	993	4·3
$6\frac{1}{2}$	8·25	3·0	535	602	543	67·4
$6\frac{1}{2}$		4·0	910	884	840	36·3
$6\frac{1}{2}$		4·5	1,000	1,028	983	36·3
$6\frac{1}{2}$		6·0	1,545	1,488	1,425	36·3
7	8·9	5·0	1,066	995	961	36·3
7		6·0	1,250	1,263	1,222	36·3
7		6·6	1,450	1,430	1,383	36·3

It will be observed from the preceding tables that a close agreement exists between the values of the frequencies of diaphragms calculated by Prof. Lamb and those actually found by direct measurement, and for diaphragms vibrating in air the expressions given can be taken as giving the true frequency within 2 per cent. In the case of diaphragms vibrating with one side immersed, a similar agreement between theory and experiment was observed particularly with the smaller type of diaphragm.

With the thicker $6\frac{1}{2}$ in. and 7 in. types, however, it was found that the observed frequencies were all considerably higher than was anticipated from the theory. A closer examination of the frequency of a large number of $6\frac{1}{2}$ in. diaphragms was therefore carried out with the results plotted in Fig. 3. Curves *A* and *B* represent the theoretical variation with thickness of the frequencies in air unloaded and with 56 grs. load. *C* shows the variation in water with the same load.

The values of the frequencies actually found in water are plotted and lie on a line corresponding more nearly to a value of

$$\beta = 0{\cdot}546\ \frac{\rho}{\rho_1} \cdot \frac{a}{h} \text{ rather than } \beta = 0{\cdot}669,\ \frac{\rho}{\rho_1} \cdot \frac{a}{h}$$

The thickness of each diaphragm was measured with a large micrometer to the nearest $\frac{1}{10}$ millimetre, but by calculating indirectly from the frequency in air it was possible to estimate the thickness to 0·01 mm. This method, however, was only

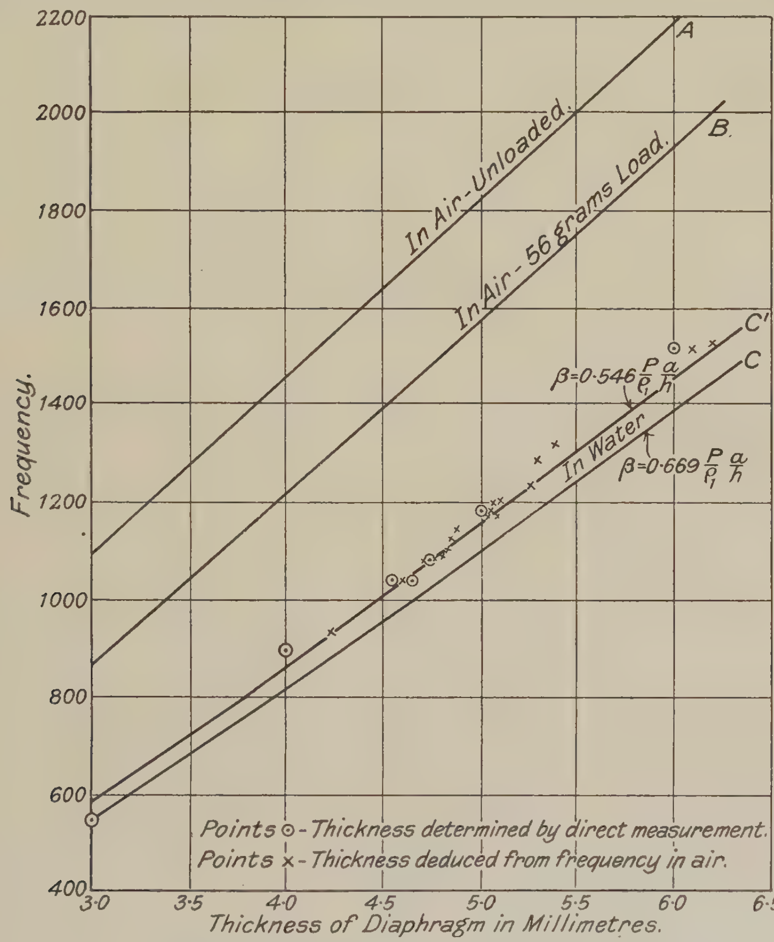


FIG. 3.—FREQUENCY OF $6\frac{1}{2}$IN. DIAPHRAGMS IN AIR AND WATER.

used when it was required to produce a diaphragm accurately tuned to a particular frequedcy.

EFFECT OF LOAD.

The effect of adding a load at the centre ot a diaphragm was fully examined and in this case the value for the increase in the inertia was found to agree

remarkably closely with that found experimentally. Fig. 4 illustrates the effect of applying additional loads up to 113 grams at the centre of a $6\frac{1}{2}$ in. diaphragm whose normal load is 67 grams. The loads consisted of flat discs of lead of diameter equal to that of the boss and screwed tightly down upon it. The values of the frequency anticipated from theory are plotted as a continuous curve while the experimental values are given as points—and the close agreement between theory and experiment in this case is at once apparent.

Higher Harmonics.

Attempts were made to observe the more complicated modes of vibration of the diaphragms, but owing to very considerable variations in the behaviour of individual diaphragms it was impossible to formulate a definite rule. However, as

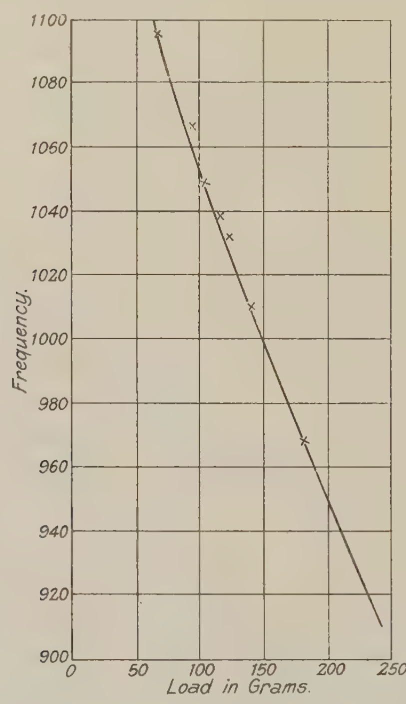


Fig. 4.—Effect of Load at Centre of $6\frac{1}{2}$in. Diaphragm.

a result of a great number of observations of many diaphragms, a first harmonic frequently occurs in water at a frequency $1\frac{1}{2}$ times that of the fundamental, and a second harmonic at about double the frequency. These frequencies do not fit in with Prof. Lamb's calculations either for vibrations with a nodal circle or nodal diameter, which give much higher values, but as the occurrence of upper harmonics was very capricious, an agreement was hardly to be expected, particularly when a relatively heavy boss was attached to the centre of each diaphragm and must have exerted considerable control on its mode of vibration.

Part II.

On the Effect of Pressure on the Vibration of Diaphragms.

The object of the present research has been to determine to what extent the frequency and general behaviour of a diaphragm as a receiver of sounds in water is governed by its depth of immersion, in other words, by the external pressure applied.

The diaphragms examined were those described in detail in the first part of this Paper, and for testing purposes were clamped into their massive steel holder—which was covered by a thick steel plate and compressed air admitted, the pressure being read by means of a gauge as indicated in the diagram Fig. 2. The large 6½ in. diaphragms of the special massive type already described were also tested and were simply arranged for the application of pressure by screwing on the stout cover plate forming part of the diaphragm unit.

The method of investigating the frequency of the diaphragms was precisely that described in Part I, the apparatus being lowered below the surface of the water in the tank and excited by the sounder of controllable frequency exactly as before.

Distortion of Diaphragms under Static Pressure.

The bulging of the diaphragms under static pressure was studied by means of an index attached to the central point, the motion of which was observed through a reading microscope.

The displacement of the centre of different diaphragms at various pressures is shown in the following table :—

Diaph.	Zero Pressure.	5 lb.	20 lb.	35 lb.	50 lb.
Steel—					
3 in. 1·0 mm. ...	0	0·083 mm.	0·257 mm.	0·431 mm.	0·647 mm.
3 ,, 1·2 ,, ...	0	0·058 ,,	0·117 ,,	0·175 ,,	Distorted
4 ,, 1·0 ,, ...	0	0·20 ,,	0·83 ,,	1·35 ,,	1·80 mm.
4 ,, 1·2 ,, ...	0	0·13 ,,	0·46 ,,	0·74 ,,	Distorted
4 ,, 1·5 ,, ...	0	0·07 ,,	0·25 ,,	0·45 ,,	Distorted
4 ,, 2·0 ,, ...	0	0·041 ,,	0·125 ,,	0·225 ,,	0·325 mm.
Bronze—					
3 in. 0·8 mm. ...	0	0·125 mm.	0·515 mm.	0·734 mm.	Distorted
3 ,, 1·4 ,, ...	0	0·66 ,,	0·24 ,,	0·393 ,,	0·556 mm.
4 ,, 3·0 ,, ...	0	0·033 ,,	0·108 ,,	0·183 ,,	0·257 ,,

Some of these results are shown plotted in Fig. 5, from which it will be seen that the displacement is proportional to the pressure, up to a certain point, beyond which the displacement becomes less rapid. It was found that at this pressure the diaphragm had begun to receive a "permanent set" and its resonance frequencies were permanently raised; its response to its resonance frequencies beyond this point was always found to be very much impaired. Provided a diaphragm is not

strained beyond this critical point, it can be repeatedly subjected to pressure, and the displacement is always proportional to the pressure.

This is in accordance with the result given by Love,* who deduces the expression :—

$$\text{Displacement of centre of diaphragm } w = \frac{1}{64}\frac{Pa^4}{D}$$

where

$$D = \frac{2}{3}\frac{Eh^3}{(1-\sigma^2)}$$

h being in this case equal to half the thickness, and P the applied pressure.

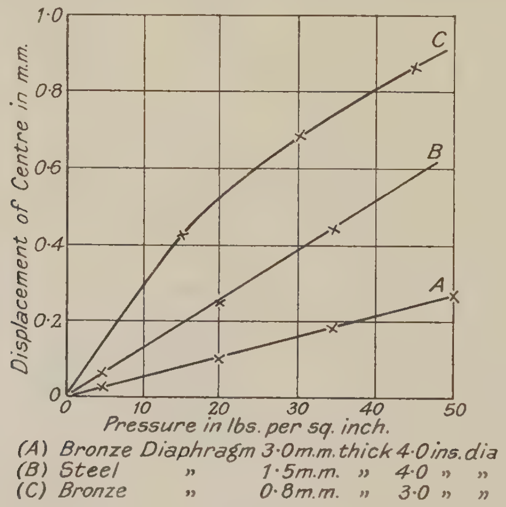


FIG. 5.

In the case of a steel diaphragm

$$w = 6{\cdot}08 \times 10^{-9}\frac{a^4}{h^3}P. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where P is expressed in lbs. per square inch.

For 4 in. 1·5 mm. steel diaphragm, w has the value 0·0238 for a pressure of 20 lb. per square inch. The value obtained experimentally in this case was 0·025 cm.

The Effect of Pressure on Frequency.

The curvature of the diaphragm, due to the pressure, causes the natural frequencies of the diaphragm to be raised in all cases, both in air and in water. The percentage rise in frequency due to pressure is approximately the same in air as in

* Love, Mathematical Theory of Elasticity, 3rd Edition, equation (83), p. 490.

water, and the aproximate equality of the figures is shown in the following table :—

Diaphragm.	Fundamental frequency.		Percentage rise at 10 lb. pressure.	Percentage rise at 30 lb. pressure.	Percentage rise at 40 lb. pressure.
Steel 4 in..........	Air	1,048 ~	1·7%	10·0%	14·3%
1·0 mm.	Water	481	2·1%	10·4%	13·6%
Steel 4 in..........	Air	1,212	1·5%	5·05%	8·8%
1·2 mm.	Water	602	0·5%	5·70%	9·9%
Steel 4 in..........	Air	1,304	To small to measure.	1·20%	1·7%
1·5 mm.	Water	892*		2·70%	3·9%
Bronze	Air	1,120	4·6%	15·4%	Distorted.
3 in. 1·0 mm. ...	Water	478	4·9%	15·7%	Distorted.

* In this case the 1st harmonic was observed.

The figures for the fundamental frequency given in the second column of the above table are the actual experimental figures. The effect of load is allowed for in calculating the percentage rise.

The percentage rise of frequency for diaphragms of different dimensions is shown in the following table ; in this table the diaphragms are all made of steel :—

Diaphragm.	Rise in Frequency. 30 lb. pressure. Percentage. Actual.	30 lb. pressure. Percentage. In terms of that of 4 in. 1 mm. diaph.	40 lb. pressure. Percentage. Actual.	40 lb. pressure. Percentage. In terms of that of 4 in. 1 mm. diaph.	$\frac{a^4}{h^4}$	Anticipated rise in frequency in terms of 4 in. 1 mm. diaph.
3 in. 1·0 mm. ...	3·47%	33·3	7·3%	31·8	$2{\cdot}10 \times 10^6$	31·8
3 in. 1·2 mm. ...	1·34%	12·9	2·8%	12·8	$1{\cdot}00 \times 10^6$	15·2
4 in. 1·0 mm. ...	10·4%	100·0	Dis	torted	$6{\cdot}63 \times 10^6$	100·0
4 in. 1·2 mm. ...	5·05	48·0	8·8%	38·4*	$3{\cdot}20 \times 10^6$	48·3*
4 in. 1·5 mm. ...	2·0%	19·2	3·0%	13·1	$1{\cdot}31 \times 10^6$	19·8
6·5 3·2 mm. ...	0·64%	6·2	1·59%	6·9	$0{\cdot}443 \times 10^6$	6·7

* The discrepancy here was due to the fact that the diaphragm was just beginning to be permanently distorted.

The theory of the vibration of a circular diaphragm when under flexure is very involved and has not been worked out, but it is important to find some simple relation to co-relate the effects of pressure in different diaphragms, as found by experiment.

The simplest relation is :—

$$\text{Percentage rise in frequency} = A\frac{a^4}{h^4}P. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where A and h are expressed in centimetres and P in lbs. per square inch, A being an empirical constant depending on the material. For steel, with these units, A has a mean value deduced from a large number of trials of $6{\cdot}0 \times 10^{-8}$.

A few further experiments were carried out using bronze diaphragms and, as would be anticipated, a rise in frequency occurred which was much greater than with steel diaphragms, being 10 to 12 times as large for corresponding diaphragms of the same dimensions.

These relations are shown by the preceding table, two columns of proportional values being included for comparison. It will be observed that they do not hold

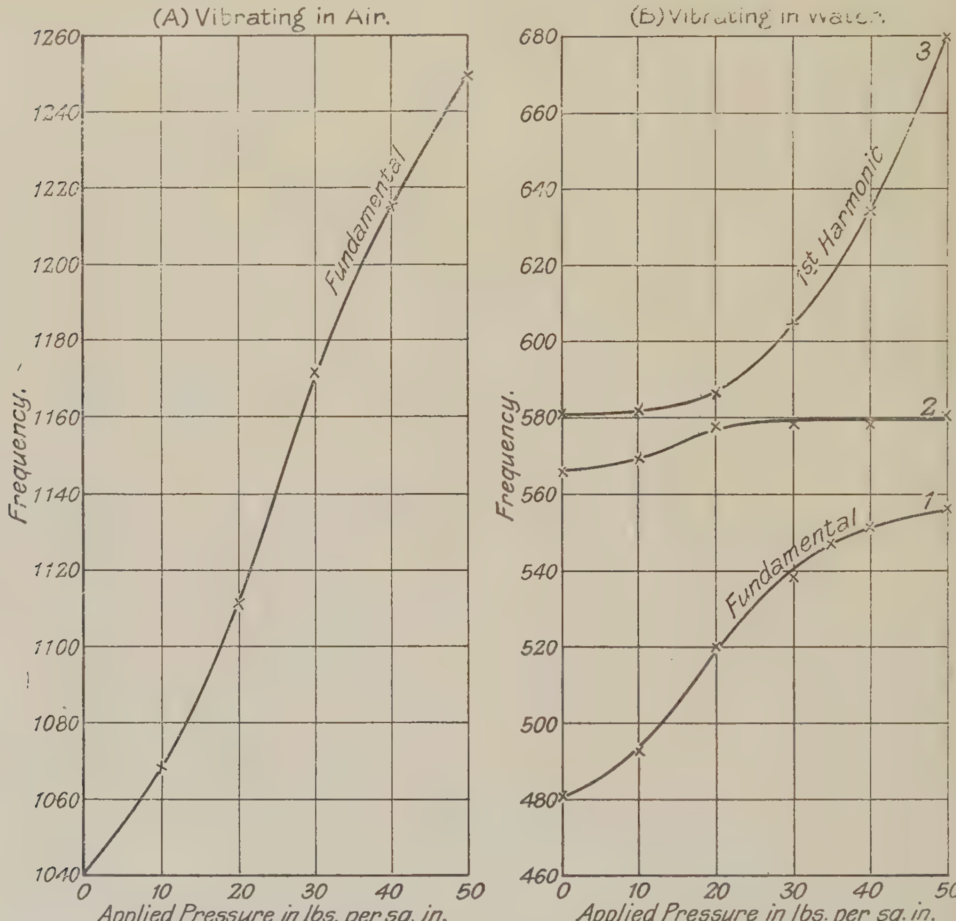


Fig. 6.—Variation of Resonance Frequency with Pressure.
Steel Diaphragm 1·0 mm. thick, 4·0 inches diameter.

when the diaphragm is strained beyond its elastic limit which is reached when the rise in frequency reaches about 10-12 per cent.; this figure seems fairly uniform for steel diaphragms of all dimensions.

General Observations.

In all cases the frequencies of the resonance points of a diaphragm are raised by increasing the pressure on one side of the diaphragm. (*See* typical curves, Fig. 6.)

The percentage variation in frequency with increase of pressure is the same in air as in water for the same diaphragm, and for different diaphragms, other things being equal, it is less the thicker the diaphragm.

The displacement of the centre of the diaphragm under uniform pressure is proportional to the pressure up to the point where the diaphragm begins to acquire a permanent "set" when the rate of displacement with increase of pressure begins to fall off.

When a resonance becomes diminished in loudness, it increases in area or extent, and when it increases in loudness it diminishes in extent; *i.e.*, becomes sharper, as though the sum of energy in the resonance tended to remain constant. In some cases the energy appears to distribute itself between different resonance points, one sound-maximum increasing in loudness at the same time that another is diminishing.

In addition to the fundamental and harmonics, smaller maxima were frequently present initially. These were not easily followed, as they behaved irregularly. They were no doubt due to local strains, &c., set up when the diaphragms were being turned on the lathe. They usually diminished or disappeared on the application of pressure. Curve 2, Fig. 6 (B), is an example of such a maximum. As the pressure was still further increased, this maximum became very faint and, at the same time the fundamental and first harmonic increased in loudness.

With the fundamental of all the diaphragms which have been examined, the curve showing the rise of frequency with pressure resembles a saturation curve, the position of the resonance point rising much more slowly with pressure beyond a certain critical pressure.

With first harmonics and others, the initial portions of the curve are similar to the initial portions of the curves for fundamentals, but the corresponding features occur at higher pressures. Curve 3, Fig. 6 (B), represents a typical first harmonic.

At the pressure at which the curve begins to turn to the horizontal, the diaphragm begins to acquire a permanent "set," and the amplitude of vibration diminishes.

Since the percentage rise in frequency for a given pressure and material varies as $\frac{a^4}{h^4}$ and the fundamental frequency varies as $\frac{h}{a^2}$, it follows that for diaphragms of the same material and different dimensions, of a given frequency (*i.e.*, $\frac{h}{a^2}$=constant), the percentage rise in frequency for a given pressure varies as $\frac{a^2}{h^3}$.

From this it is seen that a large (and thick) diaphragm would be less affected by pressure than a small (and thin) one of the same material and frequency. For example, a steel diaphragm of fundamental frequency 481 and dimensions 4 in. by 1 mm., rises in frequency at a pressure of 30 lb. per square inch, by 10·4 per cent., whereas a steel diaphragm of fundamental frequency 535 and dimensions 6·5 in. by 3·2 mm., rises in frequency at the same pressure by only 0·64 per cent.

These results are now published by permission of the Admiralty. The experiments were carried out for Naval purposes during the war under the close direction of Sir Ernest Rutherford, F.R.S., to whom the authors have great pleasure in acknowledging their indebtedness.

DISCUSSION.

Dr. W. S. TUCKER inquired whether double resonance had been observed. He had found that it was hard to avoid this phenomenon in the neighbourhood of air cavities, which form a coupled system with the diaphragm.

Mr. J. H. POWELL replied that double resonance had been observed, and that it accounted, for instance, for the form of curve 2 in Fig. 6 of the Paper. In general, however, it was avoided by suppressing air cavities.

Dr. F. LL. HOPWOOD said that much work has been done on multiple resonance, the results being analogous to those found for coupled electrical circuits. The resonance points can be varied at will by altering the coupling. No satisfactory theory has been given for overtones produced under water as it has not been found possible to deal analytically with the flow of water from one segment to another vibrating in opposite phase. It is doubtful whether the breadth of the resonance peaks has much significance, since the measurements of amplitude are scarcely reliable although the frequencies are accurately known.

Dr. A. O. RANKINE questioned the propriety of calling the overtones "harmonics." In modern music such relations may be of small importance, but for scientific purposes a distinction should be drawn between a perfect octave, for instance, and a mere overtone such as an approximate fifth or octave.

Prof. C. L. FORTESCUE suggested that the multiple resonance was due neither to harmonics nor to non-harmonic overtones, but to the coupling of two elastic systems, not necessarily including the air cavities mentioned by Dr. Tucker. The structure supporting the diaphragm would form one such system, and its being mechanically coupled to the diaphragm would perhaps account for what the authors had described as the fundamental and first harmonic in Fig. 6.

DR. CHREE, in a written communication, pointed out that the formula $p/2\pi=0{\cdot}4745hc_1/a^2$, ascribed to Prof. Lamb, is only an approximation—though a good one—the exact value of the numerical factor being 0·4693, as stated by Prof. Lamb himself. He would have expected the more exact formula to be used. He also pointed out that "iron" being so variable a substance, it would be desirable for high accuracy to determine c_1^2 directly for the actual material employed.

REPLY to the Discussion by Dr. J. H. T. ROBERTS: Double resonance was in some cases observed, but special efforts were always made to avoid air-bubbles, as it was found that the presence of even a minute air-bubble seriously interfered with the sensitivity of the diaphragm for the reception of sound from water. The behaviour of the resonances was also found to be capricious when there were minute bubbles adhering to the system.

The theory of the mode of vibration of a diaphragm under the conditions necessary for the production of overtones is very complicated, and it is possible that the apparent overtones observed (or at any rate, some of them) might be due to a mechanically coupled system, as mentioned by Prof. Fortescue. But it is unlikely that this is the whole explanation, as not only was this possibility foreseen, but cases were observed in which this coupled action was definitely taking place, and in consequence steps were taken to prevent it.

XVIII. *A Radio-Acoustic Method of Locating Positions at Sea*: Application to Navigation and to Hydrographical Survey. By* A. B. WOOD, *D.Sc., F.Inst.P.*, *and Captain* H. E. BROWNE, *O.B.E., R.N.*

RECEIVED FEBRUARY 9, 1923.

ABSTRACT.

A description is given of a series of experiments carried out to test the possibilities of the Radio-Acoustic method as a means of locating positions at sea. In this method, as applied to navigation, for example, the ship requiring her position makes a W/T " dash " at the same instant as a small charge is fired in the sea. A station on shore records the arrival of the W/T signal and also of the explosion wave at a number of hydrophones suitably disposed in known positions on the sea-bed. The times of travel of the explosion wave, and hence the distances from the charge to each hydrophone, are indicated by an Einthoven galvanometer photographic recorder.

The method permits of great accuracy and has important applications in navigation and hydrographical survey. For navigational purposes great accuracy is sacrificed to speed—it being possible to give a ship her position within a radius of half a mile within 10 minutes of receiving her request for a location. A 9 oz. charge of explosive can be located at 40 miles. In hydrographical survey work the method has already been used successfully in fixing accurately the positions of certain buoys and light vessels.

The possibility of screening and distortion effects produced by sandbanks has been investigated.

The radio-acoustic method has been thoroughly tested under working conditions. It has proved accurate and reliable by day or night, in rough or in foggy weather and at all seasons of the year. Many locations have been given by the method and no failures have been experienced.

The R/A system should be regarded as a serious competitor to the method of location by directional wireless.

I. INTRODUCTION.

THE principle of measurement of distance by means of two waves propagated with different velocities has long been known ; but it is not until recently that the method has received practical application in the determination of positons at sea. Joly† has described the method in some detail, and has proposed the use of various forms of it for navigational purposes. The underlying principle of the method is briefly as follows : The ship to be located emits simultaneous signals having different velocities of propagation. These signals are reecived in a suitable manner at a fixed station, the time interval " *t* " between the arrival of the two signals being measured.

* Work carried out at Admiralty Station, St. Margaret's Bay, Dover, under the joint direction of the Director of Torpedoes and Mining and the Director of Scientific Research, Admiralty. The authors desire to acknowledge their indebtedness to the Department of Scientific and Industrial Research for financial aid in respect of the investigation. Thanks are also due to the Admiralty for permission to publish the results of the experiments, to those who were concerned in the laying and surveying of the hydrophones, and to many others who rendered assistance and advice.

† See Phil. Mag., 36, June (1918), and Proc. Roy. Soc., A,94, August (1918).

Knowing V_1 and V_2, the velocities of propagation of the two types of signal, the distance D of the ship from the fixed station is at once obtained from

$$t=\frac{D}{V_1}-\frac{D}{V_2}.$$

Thus, if the ship originated simultaneous acoustic waves in air and in water, the respective velocities being 338 and 1,510 metres/sec., the distance D in metres would be approximately $435t$ from the receivers. If light waves and acoustic waves in air were simultaneously emitted (as in the case of a gun firing), V_1 is 338 metres and V_2 is 3×10^8 metres per second; then D is equal to V_1t practically—*i.e.*, $338t$ metres.

If two or more fixed receiving points in *known* positions are employed, it is a simple matter to locate the source of the double signal, for the observations give the ranges of the source from each of the known fixed points.

In the experiments described in this Paper a method has been developed which employs the simultaneous emission of a wireless signal in air and an explosion wave in water. The distances measured in such a case are solely dependent on a knowledge of the time interval and velocity of propagation of the explosion wave in water, for the time of propagation of the wireless wave is negligible by comparison. Recent experiments by the authors at St. Margaret's Bay, Dover, have given very accurate data for the velocity of propagation of an explosion wave in sea water.

Between the temperature-limits 6° to 17°C. and with the salinity in the neighbourhood of 35%, the velocity is expressed by the relations

$$V=4756+13{\cdot}8t-0{\cdot}12t^2 \text{ (ft./second)}$$

or

$$V=4626+13{\cdot}8t-0{\cdot}12t^2+3{\cdot}73S \text{ (ft./second)},$$

where t is the temperature in degrees centigrade and S the salinity in parts per thousand.

With this information as a basis, ranges have been measured and positions located accurately at distances greater than 50 miles.

II. Method.

The location of a source of simultaneous wireless waves (W/T) and acoustic waves is a simple application of the above method of range measurement. There is required—

(*a*) A means of producing the double signal—*e.g.*, a means of producing a W/T dash at the instant of explosion of a charge.

(*b*) Two or more acoustic receivers under water—the positions being accurately known—and a W/T receiving set at the recording station; and

(*c*) A means of recording and timing the radio-acoustic signals.

Transmission of the Double Signal.

(*a*) The radio-acoustic signal can be produced in several ways, according to the degree of accuracy required in the location or " fix."

In the simplest case, where only an approximate fix is desired, the W/T operator on the ship presses his transmitting key when he hears or feels the shock of the

explosion. In such a case the explosive may be a small charge of guncotton fitted with a fuse, the charge being thrown overboard at the position to be located. Such a method, of course, involves the personal error of the *W/T* operator, there being necessarily a certain, somewhat variable, lag between the explosion of the charge and the pressing of the *W/T* transmitting key.

For more accurate work a double key can be used, one part of the key firing an electrical detonator in the charge, the other part transmitting the *W/T* dash. If still greater accuracy is required, a simple automatic arrangement can be employed, whereby the shock of the explosion automatically closes a circuit and transmits the *W/T* signal. Up to the present the double-key method has been found quite sufficient to meet all practical requirements. In that method it is essential, of course, that a sufficiently high voltage be employed to fire the detonator in order to reduce time-lag to a minimum. Usually 80 volts are applied to the detonator, whereas 2 or 3 volts would be sufficient to fire it ; the lag in the former case is of the order of 0·001 second.

Reception of the Double-signal.

(*b*) The acoustic receivers employed to detect the arrival of the explosion impulse through the sea are hydrophones of the microphone type, laid in carefully surveyed positions. Four hydrophones are used, laid on a line approximately N. & S., just to the eastward of the Goodwin Sands. The positions are given in the following table :—

TABLE I.

Hydrophone No.	Latitude North.	Longitude East.
1	51° 24′1·5″	1° 36′38·5″
2	51° 20′26″	1° 35′27″
3	51° 16′38·5″	1° 36′33″
4	51° 12′29″	1° 35′55″

These positions are shown on Fig. 1.

The distances apart and relative orientations of the various pairs of hydrophones are given below :—

TABLE II.

Hydrophone Pair.	Distance apart, Feet.	Angle from true North (measured clockwise).
1—2	22,279	11° 43·5′
1—3	44,850	0° 26·5′
1—4	70,245	2° 15·5′
2—3	23,413	349° 42′
2—4	48,409	357° 54′
3—4	25,456	5° 27·5′

The four hydrophones are connected by twin-core armoured cable to the recording station.

Recording and Timing the Signals.

(*c*) The electrical circuits of the hydrophones consist essentially of a battery in series with the microphone and primary of a transformer, the secondary of which

is connected directly to one of the strings of a six-stringed Einthoven galvanometer set. Four of the strings of the galvanometer are connected in this way to their corresponding hydrophones, a fifth string records (through a microphone circuit) the half-second ticks of a Greenwich chronometer, whilst the sixth string records the W/T signals. The latter are received by a suitable aerial and standard receiver, amplified by six valves in cascade, rectified by a seventh valve and then passed through the line circuit of a Brown microphone relay. The circuit of the microphone of this relay consists of a dry cell, an adjustable resistance and the moving coil of a Weston relay. The resistance is so adjusted that the local circuit of the Weston relay is *just* closed, and a small current flows through the contacts and the galvanometer string. When a W/T impulse is received, the microphone of the Brown relay is disturbed and its resistance increases, causing the local contacts of the Weston relay to open and cut off the current through the galvanometer string. With this arrangement W/T signals can be recorded quite satisfactorily provided the rate of transmission of the signals is not too rapid for the Weston relay to recover in the intervals between the signals.

The photographic record (on bromide paper) is crossed by a series of fine lines marking seconds, tenths, and hundredths—for very accurate work thousandths of seconds can be estimated. The time marks are made by a phonic wheel controlled by a 50 $\sim$ tuning fork electrically maintained.

With the apparatus outlined above, a radio-acoustic signal will give the following information :—

(1) The total time interval from the charge to each hydrophone ; and (2) the time of passage of the explosion wave between each pair of hydrophones.

From (1) we can at once deduce the distance of the charge from each hydrophone, whilst from (2) we obtain a bearing of the charge from the mid-point of each pair of hydrophones.

When the method is used in navigation where rapid " fixes " are required, the total distances and bearings need only be approximate, in which case the bearing may be taken as the asymptote of the hyperbola for the corresponding time interval. In accurate work both total distances and bearings must be accurate ; consequently, all bearings must have the asymptote correction applied. The location by plotting in the radio-acoustic method permits of great accuracy in that it is in all cases determined by lines crossing almost at right angles. The bearing lines from the mid-points of hydrophone pairs, always cut the range arcs from the individual hydrophones in a large angle (comparable with 90°), whereas the intersection angles of the various bearing lines or of the range arcs amongst themselves respectively are usually small. At long ranges this point is of great importance, and indicates the fundamental reason why radio-acoustic sound ranging is so much more reliable than the three-point system, where only time-differences and therefore bearing lines only are obtained.

In the R/A method the intersecting lines which locate the explosion are mutually at right angles, whereas in the three-point method they tend at greater distances to become parallel and the location correspondingly indefinite.

III. Preliminary Experiments.

The first experiments were made with the object of testing the accuracy of the method in giving rapid and approximate locations of the charge fired by a destroyer

in known positions. Many such observations were made, charges being located at distances ranging from 10 to 40 miles from the hydrophones. In these preliminary tests the time intervals on the record were read to $\pm 0{\cdot}05$ second only, in order to reduce the total time taken to obtain a location. An average time from calling up by the destroyer to giving the position was about 10 minutes. In all cases, with certain exceptions to which we shall refer later, the position given by the R/A method agrees, within the limits of error of the destroyer's estimate, with the position of firing the charge.

During these preliminary experiments a study was made of the screening effect of sandbanks, and it may be of interest to refer briefly here to the main results obtained. Charges were fired at various points around the Goodwin Sands. In no case when a charge was fired to the westward of the sands (the hydrophones lying to the eastward—*see* Fig. 1) was any effect recorded, the explosion wave failing

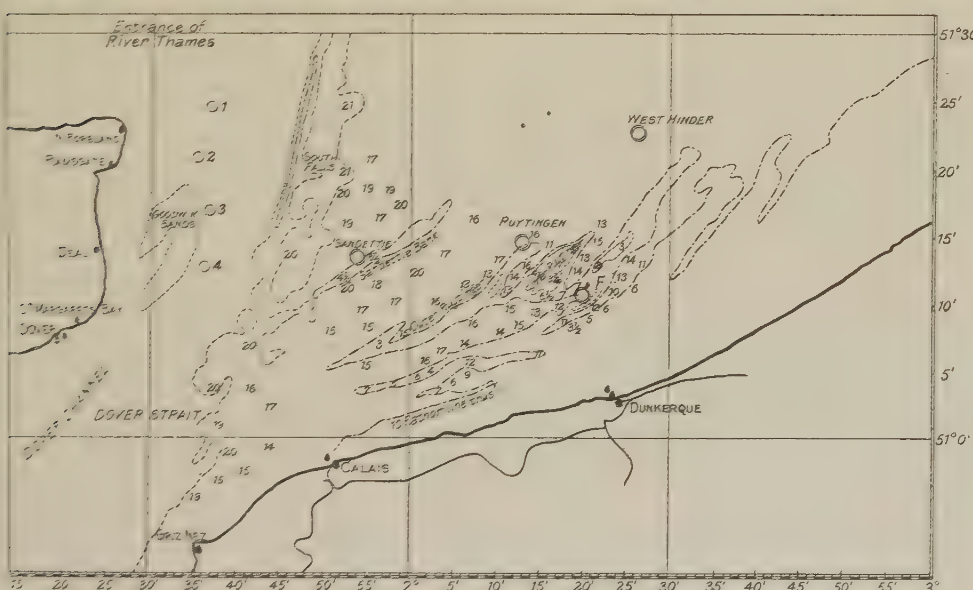


FIG. 1.

to penetrate the sands or to pass over the shallow water (3 or 4 feet) covering them. The distance of the charge in this case from the nearest hydrophone was about 5 miles, the same hydrophone having recorded the effect of a charge of the same size at 50 miles when no shoals intervened. Firing charges in other positions only partially screened by the sandbank indicated that the latter was casting a very efficient acoustic shadow. In one experiment some evidence of distortion at the edge of the sandbank was obtained, the record suggesting that an acoustic wave (of long wave length) resulting from the explosion was either diffracted round the edge of the sandbank or that the main pressure wave was retarded in velocity and changed in wave form as it passed over the shallows at the edge of the bank.

The observations in general, however, indicated the complete screening effect produced by a sandbank such as the Goodwins. A distortion effect such as that

quoted is a rare exception which would be unlikely to occur in ordinary practice, and even then would easily be detected. Other factors which influence the range of detection of the explosion wave have been examined experimentally, but the results of the investigation will be published later.

IV. Navigational Experiments.

The preliminary experiments just outlined indicated at once the possibilities of the radio-acoustic method when applied to the navigation of ships. These experiments showed that it was possible to give a vessel an accurate position within 10 minutes of receiving her request for a " fix." Consequently several trial cruises were organised in which the destroyer should proceed to any position selected by her navigating officer, and, of course, within range of the station, and ask for a " fix." The results of these experiments were entirely satisfactory, in every case the rapid R/A " fix " agreed with the position estimated by the navigating officer of the destroyer. A long list of observations of this character could be given in evidence of this agreement, but they are omitted to save space. A specimen record and location is, however, shown in Fig. 2. On one occasion the destroyer found herself between dangerous sandbanks off the Belgian coast (*see F* in Fig. 1), a mist preventing her from distinguishing shore objects. An R/A fix, however, cleared up all doubt as to her position and she returned to Dover Harbour in safety.

Such experiments as these were carried out throughout the winter of 1921-22, in very rough and in misty weather, and on every occasion the R/A locations have been found more reliable than those determined by the ship's navigating officer by ordinary methods (*e.g.*, by time-distance-bearing, or by sighting light-vessels).

There should be little further difficulty in applying the R/A method to ships in general. Most ships requiring to make a good landfall after a long voyage carry wireless apparatus. It has been found that a small 9 oz. charge can be located up to about 40 miles, and this would be a convenient size to carry.

The R/A method of locating ships at sea in foggy weather should now be considered as a competitor to the Directional Wireless Method. The R/A method possesses the very great advantage of being equally accurate and reliable day or night, at all seasons of the year and under all weather conditions—whereas directional W/T locations are often open to doubt, peculiar errors of several degrees in bearing being of frequent occurrence.

V. Hydrographical Survey.

In the foregoing remarks particular importance has been attached to the " rapid " and approximate R/A location for navigational purposes, rather than to the more accurate location which the Einthoven galvanometer record is capable of revealing.

On certain occasions when the destroyer " Thruster " asked for a " fix " whilst in the neighbourhood of the West Hinder Lightship (*see* Fig. 1), the rapid R/A location appeared to be considerably in error. The experiments were consequently repeated, the destroyer estimating her position as accurately as possible, and the Recording Station calculating the position of the charge by the accurate method. In this method the recorded times are estimated with an accuracy of $\pm 0{\cdot}001$ second, and the asymptote correction is applied to the calculated bearings. The calculated position of the charge was still found to disagree with the destroyer's estimated

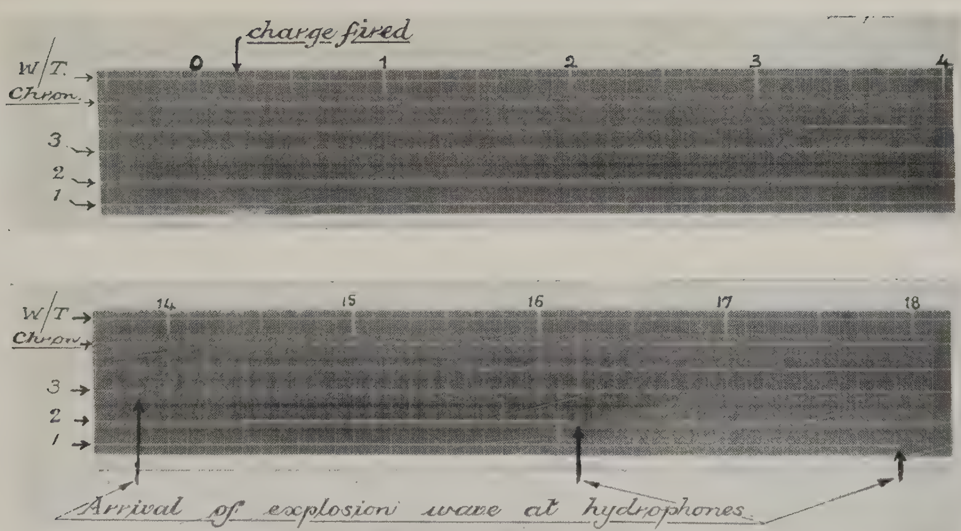


FIG. 2.—RECORD OF CHARGE NO. 54.

Charge $2\frac{1}{4}$ lbs. G.C., 40 feet deep.
Rapid R/A.—Charge 1: 15 Nautical miles.
Charge 3: $11\frac{1}{2}$ „ „
Bearing 1-2: $124\frac{3}{4}°$.
Bearing 2-3: $109\frac{1}{2}°$.

Whence position 51° 14′ 15″ N., 1° 54′ 30″ E., as compared with 51°14′ 12″ N., 1°54′ 18″ E., estimated by "Thruster."

To face page 188.

position, the divergence being much greater than the possible error in the destroyer's estimate. These observations led the authors to suspect that the light vessel was not in her charted position. Consequently further R/A observations were made in the neighbourhood of other light vessels, with the results shown in the following table :—

TABLE III.

Light Vessel.	Charted Position.	R/A Position.	Error in charted position.
"West Hinder" ...	51° 22′ 30″ N. 2° 26′ 20″ E.	51° 23′ 16″ N. 2° 27′ 50″ E.	1¼ naut. miles N.E.
"Sandettie" ...	51° 13′ 24″ N. 1° 53′ 42″ E.	51° 13′ 25″ N. 1° 53′ 43″ E.	Nil.
"Ruytingen" ...	51° 14′ 28″ N. 2° 12′ 58″ E.	51° 14′ 38″ N. 2° 13′ 59″ E.	0·7 naut. mile 78° true.

Later observations of "Sandettie" light vessel, after a period of rough weather showed her to have moved about half-a-mile from her charted position. Further observation showed that she remained fixed in this position throughout the remainder of the winter. The R/A position of "Ruytingen" light vessel was checked by Lt.-Comdr. Archer by accurate bearing observations of the light vessel and Dunkerque. The R/A position required the bearing to be 156·5° true, whilst the charted position indicates 154° true. Direct observation gave 157° true—in good agreement with the R/A prediction. This was subsequently confirmed by the French hydrographer, in response to inquiries, who gave the position of the "Ruytingen" light vessel as 51° 14′ 50″ N., 2° 13′ 52″ E., which closely agrees with the R/A location quoted in the above table. "West Hinder" light vessel was driven from her moorings in a storm soon after she had been located by R/A ; no comparison of positions has therefore been possible.

The above illustrations serve to show how the R/A method might be applied to the location of buoys, light-vessels, &c., in hydrography. It is simple, direct and time saving. An accurate "survey" location of a buoy can be worked out in about two hours by the R/A method, whereas long and laborious observations and calculations are required in other methods of survey.

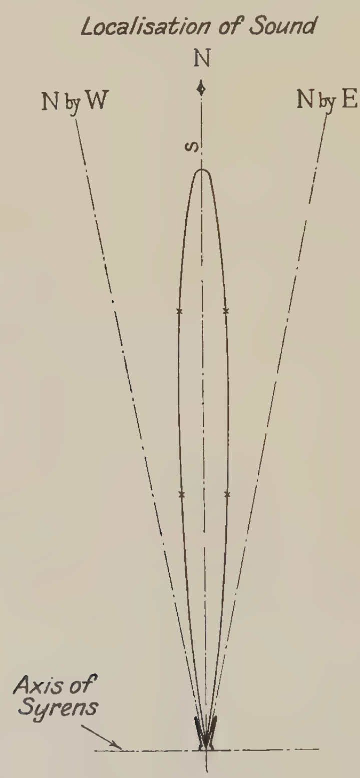
Suggestions have been laid before the Admiralty for a simple form of portable R/A set to be carried by survey ships as part of their equipment, and set up wherever required for the purpose of a particular survey.

It was considered possible that temperature differences in a horizontal plane in the sea under winter conditions might conceivably affect the R/A locations, since temperature differences of as much as 2°C. (corresponding to about 20 ft./sec. change of velocity) were known to occur. Under such conditions the wavefront of an explosion wave would to some extent be refracted, with consequent errors in range and bearings deduced from the records. Experience has shown, however, that such errors are too small to be of any importance in navigational locations, but may possibly be not entirely negligible in hydrographical survey work where greater accuracy is required. For this reason it is recommended that surveys should preferably be carried out in settled weather when temperature fluctuations are a minimum.

DISCUSSION.

Col. H. F. Towler said there was no doubt as to the great need for aids to navigation in foggy weather. The method might find a further application in the prevention of collisions by a ship's emitting simultaneous fog-horn and wireless signals for observation by other ships. The direction could be got by wireless D/F, as the sound might be subject to refraction in the conditions mentioned. This arrangement would have two great advantages over that in which the locations are given by a shore station, viz., that the navigator would feel more confidence in the observations if they were taken by himself, and there would be no risk of jamming when a number of ships simultaneously required locations in a sudden fog. The speaker considered that for navigation near the shore the wireless D/F shore stations can hold their own against the R/A stations, as the errors ascribed to the former are not usually manifested at the ranges considered, viz. 30 to 40 miles. D/F stations can give a location in as good time as that claimed for R/A (10 minutes), they already exist in considerable numbers, and they are useful for other than marine purposes, *e.g.*, for aircraft.

Mr. F. Twyman said it might not be irrelevant to mention an acoustic directional transmitting system* which he had devised, but had not yet constructed. It comprises a row of synchronised



Number of Syrens 11. Vibrations per second 224. Distance apart 2 ft. 6 in.

syrens the total length of which is great compared with the wave length, and emits sound which is practically confined to the plane bisecting the row at right angles. The distribution of intensity has been worked out by Dr. Silberstein on the basis of the ideas put forward by Lord Rayleigh.†

* British Patent Specification 4797/14 (Twyman and Another).
† Theory of Sound, 1896, Vol. 2, p. 103.

It might be expected that a beam of sound thus produced would be of more stable intensity in windy weather than a spherical wave from a point source, and that its intensity would fall off less with distance. A directional receiving apparatus might perhaps be designed on analogous lines.

Dr. C. V. DRYSDALE said that the R/A method would be an invaluable aid to navigation, but he agreed that seamen would be disinclined to rely on observations taken by other persons. The solution seemed to be that the lightships should be fitted as transmitting stations and the ships as receiving stations. For direction finding it would be preferable to use subaqueous sounds, detected by hydrophone, rather than those emitted by a foghorn, which are liable to changes of direction by refraction or reflection.

Mr. F. E. SMITH said that the captain of a vessel would certainly want to make the observations himself. It might not be generally known that, backward as the wireless equipment of this country may be, some thousands of locations are given annually to ships by the D/F shore stations. In spite of the greater accuracy of R/A, the D/F stations will probably hold the field in this service on account of their relatively small cost. Their accuracy is greatest where it is most needed, namely, in the proximity of the stations themselves. The speaker paid a tribute to the naval officers and ratings who had worked at the method and had shown great keenness as to both its scientific and its technical aspects.

Mr. T. SMITH, referring to the question of the distribution of sound intensity raised by Mr. Twyman, said that on general physical grounds one would assume that air and water are not dispersive media for sound. If, however, on a closer approximation to the facts they should be regarded as dispersive, could the existence of "silent zones" be explained on such lines?

Major W. S. TUCKER said that there are two kinds of silent zones. The first is of the type exemplified in the recent explosion experiment in Holland. The other type is really illusory, and is due simply to screening by acoustic clouds. The illusion arises from the fact that the observations have been made upon foghorn blasts emitted at one-minute intervals. If during such an interval the cloud has moved, the observing vessel having meanwhile also moved along its course, an impression is created that the vessel has steamed out of a silent zone. In reply to a question by Dr. Rayner, the speaker said that an acoustic cloud consists in the surface of separation between two bodies of air in different physical states.

Dr. A. RUSSELL pointed out that the parabolic formula given by the authors for the variation of the velocity of subaqueous sound with temperature showed a maximum. Was the formula a purely empirical one?

Mr. R. S. H. BOULDING (communicated): The authors are to be congratulated on having carried out some very interesting experiments which should prove extremely useful to the Mercantile Marine. In regard to recording, would it not be preferable, if possible, to operate the galvanometer by the W.T. signals without the aid of a Brown microphone relay and a Weston relay? The statement that a rough position can be given in ten minutes seems to suggest that there is room for improvement. Admittedly, accurate determinations require careful calculation, but rough locations could, one would think, be worked out graphically or instrumentally in a negligible time. I should very much like to have any information the authors can give as to the time lag introduced by a sound wave passing round the edge of a sand bank. I gather that this rarely occurs; but if it does, it constitutes a somewhat serious objection to the R/A method, unless it is possible for the observer to ascertain without doubt exactly what is happening. Although quite a small explosive charge is sufficient, it is feared that the use of an explosive of any sort may be objected to in connection with the Mercantile Marine. I have obtained satisfactory results with a submarine bell up to distances of 10 to 15 miles. The comparison between the R/A position of the Ruytingen L/V and that given by the French hydrographer is interesting. The difference between these two positions appears to be some 500 yards. What degree of accuracy is claimed for the R/A method? I should like to ask whether any allowance has been made for tidal streams which, in the Straits of Dover, reach at times velocities 8 ft. to 10 ft. per second.

REPLY to the Discussion by Dr. A. B. WOOD: The method mentioned by Col. Fowler for the prevention of collisions at sea by the simultaneous emission of foghorn and W/T signals has already received attention by the authors. On account of the great variability of atmospheric conditions, however, it is considered more satisfactory for the lightship to emit simultaneous *under-water acoustic signals* and W/T signals. A proposal on these lines has already been

submitted to the Admiralty by the authors* in which the light-vessel emits a series of W/T dots (the first of which is accompanied by the acoustic signal) at intervals corresponding to half a mile distance from the light-vessel (*i.e.*, every 3/5 second app.). A ship can thus obtain her distance from the light-vessel by counting the number of W/T dots received up to the arrival of the acoustic signal. The direction can, of course, be obtained by W/T.

Light-vessels are liable to shift position in stormy weather, however, and this might have serious consequences under certain conditions.

The R/A method has one great advantage over the directional W/T method in that it gives not only bearings, but also ranges. An error of 2 or 3 deg. in bearing might result in an error of several miles in range of location, especially so when the position of the vessel lies on a line making only a small angle with the base-line (the transmitters in W/T and receiving hydrophones in R/A). A knowledge of the range as well as the bearing is essential under such circumstances.

The time of 10 minutes estimated for giving a location is largely occupied by the W/T procedure of calling up, &c., which is common to all systems of this sort and cannot be reduced appreciably. One or two minutes might, however, be gained by the use of more automatic methods of reading off the time-intervals and plotting the positions.

(To Mr. F. Twyman): The method of directional acoustic transmission in air as suggested is certainly very interesting, but experience has shown that the transmission of sound in air over great distances is very unsatisfactory both as regards distortion of wave-front and erratic variation of intensity. Unless extremely powerful sources are employed the range is limited to a few miles only. Under the best conditions it is considered that an accuracy of ± 5 deg. in direction could hardly be obtained by this method. The consequent errors of location would under such circumstances become very serious. Directional reception of under-water signals has already been considered and abandoned mainly on account of such serious errors in direction-finding.

As we have already stated above, the main feature of the R/A method is that *it gives an accurate value of the range in addition to accurate bearings.*

(To Dr. C. V. Drysdale): There is much to be said in favour of the "converse" R/A method. Owing to the closing down of the St. Margaret's Bay station, however, it was not possible to continue experiments on these lines.

(To Mr. F. E. Smith): It should be noted that in W/T direction-finding the captain of a vessel does not make the observations personally any more than in the R/A method. In the latter case it should be noted the record is taken in permanent form so that locations can be checked, complaints can be investigated and reasons ascertained. This is not possible in the W/T D.F. method. In the R/A method there are no "night-errors" or any such errors corresponding. The difference of cost in the two systems worked on a commercial basis need only be very trifling.

(To Mr. T. Smith and Major W. S. Tucker): The dispersive and distortional effects of a non-homogeneous atmosphere is the most serious objection to the use of acoustic signals in air as a means of direction-finding. The sea is a much more homogeneous medium in which sound waves are propagated for very great distances without serious distortion of wave-front. No "silent zones" have ever been observed in the sea, except perhaps in the very shallow water above a sand-bank such as the Goodwins.

(To Dr. A. Russell): The velocity formula referred to is an empirical one which applies only within the range of temperatures 6°C. and 17°C., between which limits the velocity measurements were made.

(To Mr. R. S. H. Boulding): In recording W/T signals both methods have been employed, but for certain experimental reasons it was found preferable to use the relay arrangement.

With regard to the time of 10 minutes required to obtain a navigational "fix," *see* reply to Col. Fowler.

On only one occasion has any distortion effect by a sand-bank been observed, and on that occasion the experiment was deliberately arranged to determine the extent of such distortion under extreme conditions. The record obtained was quite normal as regards the three hydrophones clear of the sand-bank, but in the case of the fourth hydrophone the record was only just readable and was obviously different in character from the usual sharp "break" produced by an explosion. Under service conditions such a record would have been rejected

* *See* also Joly, loc. cit.

without hesitation and the "fix" given to the ship would have been based entirely on the information supplied by the three perfectly readable breaks. In *all* cases the ranges were good, but the bearing obtained by using the fourth record was clearly in error. This point was only mentioned in the Paper as being of academic interest—even under the worst conditions such an effect could certainly not be regarded as serious in the service application of the R/A method.

It has been definitely decided by the Board of Trade that, subject to proper precautions, no objection will be raised to the carrying of the necessary explosive bombs by merchant vessels.

The accuracy of the R/A method as applied to survey purposes is very great. Since the velocity of the explosion wave is known with an accuracy of 1 or 2 ft. per second at all seasons of the year,* it is considered that the error in range at a distance of 50 miles does not exceed ± 250 ft. as an outside estimate. The difference in position of the Ruytingen Light Vessel obtained by R/A and by the French Hydrographer might possibly be ascribed to the fact that the observations were made at different times and the light-vessel had shifted in the meantime, or what is more probable, the position given by the French Hydrographer (obtained by ordinary laborious survey methods) is in error by the amount stated.

In accurate survey work it is recommended that all observations be made as far as possible at times of slack water (neaps preferred) when no tide is running. Otherwise a correction for tidal velocity must be applied to the explosion wave-velocity.

* *See* Proc. Roy. Soc. loc. cit.

DEMONSTRATION of an Electro-Capillary Relay for Wired Wireless. By Major C. E. PRINCE, *O.B.E.*

THE relay is intended for use with a calling device in connection with high-frequency currents acting as carrier waves for telephony over power-mains. The high-frequency current is rectified and passed through a thread of mercury which is contained in a capillary tube, and is in contact at each end with a quantity of acid, platinum wires by which the current enters and leaves being immersed in the latter. The passage of the current causes the mercury thread to move, and it was suggested many years ago by Wheatstone that this thread might be made to close an electric circuit ; but practical difficulties prevented the successful application of the idea. In the present invention the capillary tube is arranged horizontally on a beam which, as soon as the mercury moves, over-balances in consequence of the weight of the latter and closes the circuit of a call bell or lamp. To prevent the evaporation of the acid the cups containing it are connected by a further capillary tube which serves to equalise the pressure, and the tube in which the mercury thread lies is widened at its middle point to form a chamber for the reception of bubbles and other obstructions which may have got into the tube before sealing.

The circuit arrangements make provision for the restoration of the mercury to its original position whether the call be answered or not. In series with the thread and with a rectifier is arranged a condenser in which the charge that has passed round the circuit is stored, and after the call this charge is sent through the mercury and acid in the reverse direction. The required discharge is effected by contacts which short-circuit the rectifier when the switch-hook is raised, but if the call be unanswered the same result is produced more slowly by a high-resistance leak.

The instrument will respond to currents of 4 or 5 or even 2 microamperes, and will function during long periods without attention. Its sensibility was demonstrated by passing through it the voice-current from the secondary of a microphone transformer, and subsequently the current from a single dry cell in series with the body of the demonstrator. If the leak resistance be small enough Morse signals will fail to effect a call in consequence of the leakage which takes place during the intervals between the signal elements, while a series of long dashes will nevertheless be effective and can be used as a call signal.

The cause of the movement of the mercury is obscure, and the phenomenon is of some interest to physicists. The total movement appears to be proportional to the coulombs which pass, so that the arrangement might be used as a quantity meter for small currents, such as voice currents.

DÈMONSTRATION of the Flamephone (Scientific & Projections, Ltd.) By Mr. H. W. HEATH, *B.Sc.*

THE apparatus constitutes an improved gramophone, and employs a gas flame to improve the quality and intensity of reproduction.

The sound-box is divided by its diaphragm into two chambers, one of which communicates with a small horn while the other communicates with a supply pipe through which coal gas passes to a pair of vertical thin burner tubes. The tubes are perforated with a series of holes from which the gas issues, the jets projecting over the mouth of the horn. On lighting the gas an increase in the volume of sound can be observed, and also a marked improvement in quality, the notes of lower pitch being accentuated. This effect has not been completely explained.

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